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# THORACENTESIS,

AND ITS

## GENERAL RESULTS

DURING TWENTY YEARS OF PROFESSIONAL LIFE.

REMARKS MADE AT A STATED MEETING OF THE NEW YORK  
ACADEMY OF MEDICINE, HELD APRIL 7, 1870.

(BY INVITATION.)

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# THORACENTESIS, AND ITS GENERAL RESULTS.

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MR. PRESIDENT AND GENTLEMEN OF THE ACADEMY:—On this occasion I think it best to be entirely frank. A few weeks ago, on the receipt of a letter from your colleague, Dr. Leaming, asking my present opinion on the subject of Thoracentesis, and whether I still approved of performing it, as I formerly did, I replied that not only was I more than ever confirmed in my opinion of the value of the operation, but that I wished that your meetings were to be held at a reasonable distance from Boston, and I might crave permission to be allowed to attend one, in order more clearly to present the claims of this method of treating pleuritic effusions.

Upon this hint, your President, my almost life-long friend, Dr. Bulkley, sent me an invitation, and urged me to forget the distance. Friendship, therefore, has summoned me. I have, however, come in obedience not only to that, but likewise to two of the noblest axioms of our profession: viz., 1st, The profession claims of every member a full and frank declaration of all that is valuable in his experience; and 2d, It becomes the duty of every one to act fully up to this sacred rule, in order that, by so doing, we may all become better able to alleviate human suffering, or perchance save human life.

Some of you may perhaps remember that I have already published papers on the subject of Thoracentesis, viz., in the *American Journal of the Medical Sciences*,\* the *American Medical Monthly*,† and the *Boston Medical and Surgical Journal*‡

My first article appeared in 1852; my last in 1857.

Notwithstanding these publications, in order to give you a clear idea of the progress of my mind, and to show that my present views are the results of no sudden enthusiasm, but rather of many years of thought and of practical experience, I think it best to take a very summary review of the subject of thoracentesis, and of my connection with it. I shall do this with sincere pleasure. I rejoice to have the opportunity of doing so before this Academy. For, be assured, I know of no one thing in my professional life for which I more sincerely thank God, than for the fact that I was early led to perform this operation as freely as I have done it; for by it I am sure I have relieved many from severe suffering, and in some instances, I believe, I have saved human life.

Thoracentesis has been known from the time of Hippocrates,

\* *American Journal of the Medical Sciences*, April, 1852.

† *American Medical Monthly*, Jan., 1853. New York.

‡ *Boston Medical and Surgical Journal*, May 25, 1857.

but it was used only, or almost only, when nature, by local prominence, had selected a spot for an operation to be made, by herself or by the surgeon. In other words, when an abscess was formed the surgeon simply lanced it, or cut down upon and opened it cautiously with his bistoury. Not until Laënnec made his invaluable discovery of auscultation, could a fair diagnosis of cases be made. By this discovery a large class of cases were laid open before us which were wholly unrecognized before. In these previously unknown regions of diagnosis now lie, and will forever hereafter lie, most of the fairest cases for thoracentesis. But even now the majority of the profession do not know how to avail themselves of the knowledge thus given, or they dare not avail themselves of it, even to relieve an untold amount of suffering. Men who would not hesitate to plunge a trocar into, or even to lay freely open, an abscess lying under the peritoneal walls, will refuse to attempt to alleviate orthopnœa threatening death, arising from the accumulation of fluid in the thoracic cavity.

On the continent of Europe, about 1840, Trousseau was almost, if not quite, alone in recommending thoracentesis. He had no fear of performing it—but he used a much heavier instrument than that which I have always used—an instrument better fitted, in fact, for the side of a barrel than for the human chest. Vallex\* was at the same time opposed to it—and doubtless Vallex expressed medical opinion, while Trousseau was the prophet of the future!

In England, no one heartily defended the operation until Messrs. Hughes and Cock published cases of successful result in *Guy Hospital Reports*.†

But it may be fairly asserted that the profession in Europe, as a body, ignored it altogether, and thereby, as I think, have allowed some persons to die who might have been saved—or they violently opposed it because most cases, when left to the slow and tedious process that nature at times sets up, and which at a late hour have been tapped, usually turned out badly. By the operation I shall suggest, I trust that hereafter we shall not wait for nature. We shall rather direct than follow her.

In this country we have, until of late years, followed the guidance of Europe; and the terse and sharp remark made to me (when I first began to operate) by one of the most noted auscultators of America fully expresses the real state of medical public opinion. From what I knew of his conservatism, I anticipated that he would object to it, and I was not surprised, therefore, when, to my rather quiet and deprecatory question, as to what he really thought of the value of the operation, he quickly replied, "I would as soon send a bullet into the chest as plunge a trocar into it." Evidently, to his mind, death was equally liable to occur from either operation! Though rather strongly stated, I cannot but think that this remark was about the result to which medical opinion had arrived in this country. I well re-

\* *Guide du Médecin Praticien*, &c., vol. i., p. 564. Paris, 1850.

† *Guy Hospital Reports*, April and Oct., 1844.

member that when Dr. Wyman first performed his brilliant operation with the exploring trocar and canula and suction-pump,—although the patient rapidly recovered from impending death and terrible distress,—the cry of eminent surgeons in Boston was not at the success, but rather the “imprudence of the operation.”

Before I saw Dr. Wyman’s method, I had thought much upon the subject, and for years had to fight against the proposition laid down by my excellent master in medicine, Dr. James Jackson, that nature will generally carry off fluid from the pleura, provided there be no serious complication, such as tubercle, in the case. Doubtless the majority of cases of pleurisy go through these various processes towards cure, even with the simplest, or with no treatment; but there are, I am sure, numberless cases in which thoracentesis will hereafter be used with success which heretofore have been allowed rapidly or slowly to go on towards death. Many years ago I was convinced of the truth of the latter part of these remarks by the following cases:—A sailor at Chelsea Hospital had been ill for many months with what, *at that time*, was an obscure affection of the chest. It was supposed to be some malignant disease. I was very desirous that the surgeon should plunge in an exploring needle. It was contrary to the rules of surgery, and he shrunk from the proposition. The patient died slowly, worn down by the disease, and at the autopsy an enormous quantity of serous fluid was found in one pleural cavity, without a trace of disease in the other parts of the body.

Soon afterwards I saw at the Massachusetts General Hospital a young woman who, in previously perfect health, had acute pleurisy with a large effusion. I watched it under the care of another, and after months of illness, phthisis set in and she died. The thought occurred to me whether relief of severe symptoms might not have been obtained, or *perhaps even the phthisis itself prevented*, if thoracentesis had been performed in the early period of the disease. That tubercles were consequent, not precursors of the disease, I now, under the light of modern medicine, fully believe, as, in fact, I really thought at that time.

Finally, about 1848, another case of a similar character occurred under my own care, when attending physician at the hospital. I called a consultation of my colleagues, and asked if an operation should be performed. An unanimous “Nay,” with the exception of my own affirmative vote, was the result. I then determined I would ask no further consultation, but would take the responsibility, and ask the surgeons to operate for me.

Not long afterwards, a case of chronic pleurisy came to the hospital, and though an unfavorable case, I asked the late Dr. J. M. Warren to operate, and he did so in the presence of his father. It was done in the usual mediæval\* manner, viz.: by cutting

\* This expression, I fear, is somewhat too severe, if applied to all cases. The excellent remarks and surgical cases quoted by Dr. Teale, who saw the happy results of such an operation in not a few cases of gun-shot wounds during the late rebellion, fully prove that the operation, though, as

slowly down and gradually opening the pleura. It left a large gaping wound; and although pus was discharged, the patient did not rally much, and soon afterwards died.

Consequently, I decided that unless surgery could do better than that, I would none of it. I was satisfied, however, that a way would be found to treat such cases. About this period I read the facts recorded by Messrs. Hughes and Cock, and spoken of above. In 1849 I was called to see a young child who had one pleural cavity *apparently* but half-full of fluid, and yet had had one or two *attacks of excessive orthopnoea*. I asked a surgeon to consult with me about using the common trocar. At our visit the child was sitting apparently so easy in his mother's lap, that, on mature consideration, we determined to wait until dyspnoea should again set in. Never was there a more fatal error. I should not wait again in such a case. The child died in a fit of dyspnoea the night following our consultation. I asked permission to plunge a trocar into the affected side, which fully confirmed the diagnosis by allowing pus to escape. Allow me, in anticipation of my subsequent experience, to say, that I doubt not that we often mistake in our estimate of the *amount* of fluid in the chest: we are misled by the tympanitic resonance of the upper part of the thorax, which may happen even when a large quantity of fluid is contained in it.

In 1850 occurred my first case, in which Dr. Morrill Wyman used the exploring trocar and canula with suction-pump attached, and which I immediately saw was the apparatus I had long sought for. That apparatus I have modified somewhat, so as to make it, I think, more convenient; but the principle of the instrument remains as suggested\* by Dr. Wyman.

The patient was a young house-painter, who had fallen from a certain height about five or six weeks before I saw him. Cough had set in, with copious sputa and emaciation, &c., and many of the symptoms of phthisis, in fact. He was supposed by physician and friends to be dying of rapid consumption. The prominent symptoms when I saw him were orthopnoea and emaciation, and great distress; and his pulse was at 120. I found *positively* effusion into one pleural cavity, *possibly* tuberculous disease of the lung; but of the latter part of the proposition there was *no proof*. I suggested that the only remedy to be tried first was thoracentesis. The friends obstinately refused to allow it to be done. The man had made his will and taken leave of the world, and they undertook to say that nothing should be done. I stated my opinion to the patient, and he, despite of opposition, decided to allow me to do whatever I thought best. Fortunately, I had heard of the very successful case under the care of Dr. Wyman,

I still think, rather unadvisable in cases of pure chronic pleurisy, may be very useful in some cases of fluid in the cavity of the thorax, or in cases of injury of the thoracic wall, with subsequent formation of pus.

\* By this instrument we give less pain and leave no wound, which cannot be said of any other apparatus before employed.



and I therefore summoned him in consultation. He agreed with me. The operation was attempted, by introducing the trocar below the angle of the scapula. No pus followed or could be drawn. We left discomfited. I examined the patient the next day, and becoming more convinced than ever of the fact of fluid being in the chest, I again summoned Dr. W., and stated that I believed he had failed because he had introduced the instrument *too slowly* and *not deep enough*. Accordingly, although the patient nearly fainted as we placed him in the chair, the operation was again done with a more rapid and deeper thrust, and this time with entire success. Nine ounces of pus were withdrawn, with great relief. He immediately began to recover. Nature, aided by the tonic course pursued, gradually but steadily removed the rest of the pus. In two months he was at work again, and within a year afterwards no difference could be found on auscultation between the two sides of the chest!

Shrinking from all surgery, I yet felt compelled soon after this case to operate myself. This I have done, and what was at one time an object of dread has become a mere matter of course, because I have never seen any evil, or even any very distressing symptoms resulting from it.

From 1850 to April 1, 1870, I have tapped at least 248 times\* on 153 persons. I have *counted* thus much, but have not yet made accurate and entire examination of all my records.

Surely this amount of experience by any one deserves attention from the profession. It may well be asked whether we all avail ourselves sufficiently often of this simple, innocuous operation in the following contingencies:—

1st. To save life when immediately threatened.

2d. To prolong life, even when complicated with severe disease.

3d. To shorten latent pleurisy.

4th. To give temporary relief merely, in absolutely hopeless cases.

5th. I think we shall hereafter operate on cases of common pleurisy which do not early yield to remedies after a few weeks of treatment.

To each of these propositions let me now draw your attention, by the citing briefly some of the cases that have come under my notice, which may illustrate each proposition.

*First.—To save human life when threatened with excessive permanent dyspnoea, or by single severe paroxysms of the same.*

I cite the following case, already quoted in one of my published papers, as one of the most remarkable:—

A woman, four and a half months pregnant, had been suffering with very violent pleurisy of the left side, accompanied with great orthopnoea. When I saw her she was believed to be near death. Her countenance was haggard and anxious, and the dyspnoea was

\* Since the meeting of the Academy I have operated twice more, making at least 250 tapplings in 20 years on 154 persons.

extreme. The pregnancy was a serious complication. Nevertheless I tapped her, and drew off pus with infinite comfort, and with relief to all the prominent symptoms. I was obliged to tap four or five times before the termination of pregnancy, owing to the recurrence of severe symptoms. She was well delivered of a living child at the full term. Afterwards I made a permanent opening. Finally she recovered, and is now living, with chest contracted, as is usual in chronic pleurisy. I believed, and still believe, that the operation several times saved her from *immediately threatened* death.

In November, 1865, I was called to a young man with orthopnea, and with most of the signs of phthisis. It appeared that during October he had been unwell, and a pleuritic effusion had been recognized on 29th.—Nov. 10th, orthopnea came on, and he became very ill. I removed a quart of sero-purulent fluid that day, with great relief; but on the 16th the symptoms were again severe, and I removed three and a half pints with equal relief. Dec. 8th, the fluid having reaccumulated, I made a permanent opening. Under this he was relieved of the dyspnea, but he gradually became anemic, and finally hectic began. Under these circumstances, although there was a free discharge (apparently) of pus, we decided to inject daily warm water into the cavity. This produced, when first performed, the discharge of a very large quantity of pus, that had apparently collected in a somewhat concrete form below the opening, and could not flow out. Under these injections the hectic disappeared, and the patient went on steadily towards a cure. For more than two years he has been a clerk. His chest is somewhat contracted, but his health is perfectly good.

My belief is that in this case also life was saved.

The two following cases become doubly interesting, from their apparently showing what I have no doubt is a fact,—that *we should not wait too long before operating*.

A little boy aged five years, and a little girl aged thirteen, both in humble life, and in unfavorable circumstances for health, were seized about the last of November, 1868. I saw the boy Feb. 2d, 1869, and the girl on the 31st of May following, or about four months later.

They both had the severest symptoms of pleurisy, and the boy was particularly emaciated, and had hectic fever. He could not lie down, and was supposed by his attending physician to be dying of consumption. It was about *two months* after the commencement of the disease that I operated on him. It was *six months* when I tapped the girl. Both had pus in the chest. In both a permanent opening became necessary, after having been twice operated on with the exploring trochar. The boy presented the most perfect result in a complete restoration not only to health, but to complete symmetry of the chest, and of all the respiratory functions; so that several months afterwards no difference could be perceived between the two lungs. The girl,



on the contrary, though better than before the operation, has had always a fistulous opening in the side, and fears are entertained that phthisis will eventually come on.

The boy is now the picture of robust, rosy health. The girl, though better in health than before the operations, still bears on her whole aspect the marks of long-continued disease.

Allow me, in passing, to notice in the case of the little boy the *natural language*, so to speak, of the great relief usually arising from this operation. At the times of the second and third operations, it was that of the satisfaction felt by the little fellow at the thought that the operation was destined to relieve him.

At my first operation he was very violent, and cried bitterly ; but as he gained so much comfort from it, he made not a whimper when he saw me prepared to operate a second time, and when his father took him upon his knee, previously to the puncture. At the third operation he looked up at me with a most piteous yet confiding expression of face, and said : " You will please hurt me as little as you can, won't you ! " I mention this fact in order to prove by the natural language of the child, that what adults express in words and acts when they *with avidity beg me* to operate again, after having once experienced relief, so the youngest children show the same thought in a simpler style.

I operated before the medical class (Feb. 2, 1866) on a very stout, burly Irishman. He had been ill six months, and the signs showed one pleural cavity apparently full. It had come on insidiously, the principal symptom being a gradually increasing dyspnoea. I tapped him ; for I deemed that the first thing to be done, as he had been so long ill. Only about a drachm of serous fluid ran out before applying the suction-pump, and not a drop more could I get with the instrument. Evidently there was something obstructing it. Doing as I had done in one or more cases, I reversed the operation of the pump, and injected about an ounce of warm water. But it was too much for him in his nervous state to bear. The small extra pressure introduced into his already *distended* chest produced greatly increased dyspnoea. He became violently agitated—with some lividity of the lips—and roughly declared that he could not bear it. I instantly withdrew the trocar, and remarked to the class that I must operate higher up. These gentlemen were looking on rather in wonder, and perhaps suppressed merriment, at the apparent failure of the operation I had praised so much. The man instantly said, " I'll be d——d if I will be murdered in this way. Let me go." Leaving him to his meditations and his mutterings, I turned to the class and said : " Gentlemen, we have proof conclusive that this man's chest is full of a thin fluid. It is equally evident that thus far we have been foiled in getting it out. But ought we in duty to him to cease from our endeavor ? Decidedly not. Probably some lymph lies down where I have punctured ; and the only thing to be done is to put the instrument in higher up. That is what I propose to do ; but the man, like a foolish child, under-

takes to say I shall not. Very well, let him die then, if he please, but I shall do nothing else, unless I tap him first. He has been treated long enough with medicine, and you see the result. If he thinks that I operate to amuse you, or to please myself, or for any other reason than duty to him, he is mistaken. Under a solemn sense of that duty I commenced the operation, and nothing yet has happened to convince me that I ought not to go on with it. In conclusion, I say to you that if any one of you at any future time be placed in similar circumstances, and do not propose to pursue exactly the course which I suggest now, he will prove himself to be either an *ignoramus* or a *coward*. Meanwhile, I have no right to bind this person, as I would a baby in similar circumstances. He is a man in stature; and if he refuses, I will let him go and die, if he wishes so to do. I wash my hands of all responsibility." During these remarks, though I paid no *apparent* attention to my patient, I noticed a change of manner, and at the end of the talk he gruffly said: "You may do it." Accordingly, I introduced the trocar an inch or more higher, and nearer the line of the axilla, and easily drew off about five pints of serum, with great relief, and with the effect of producing entire confidence in me, and the usual willingness evinced by all patients to have a second and third operation. Such were necessary in his case. The lung expanded very slowly; and months afterwards I found great flatness on percussion, and only a slight crumple could be obtained on the fullest breath all over the affected side. Evidently the superficies of the lung could not expand.\* Possibly results may happen in his case similar to those which I have noticed in two of the whole number operated on, viz.: a gradual enlargement of the heart, with excessive palpitation and dyspnœa on exertion or exposure to a violent wind. In these attacks these two have died after months of comparative comfort, provided the attacks could be avoided, but during which paroxysms the patient suffered extremely as in cardiac disease.

In August, 1866, I was summoned hastily to a neighboring city and found a woman recently delivered propped up for excessive dyspnœa, and apparently threatened with death, unless relieved. The attack was acute pleurisy, occurring while she was in the puerperal state. I drew a large quantity of pus with an amount of relief, as great in fact as one sees after the last pain of a severe labor. But though the severe dyspnœa did not return, there was great prostration, and fear arose two days afterwards that she would not recover. From that partial collapse she gradually rallied, and afterwards went on steadily towards recovery, so that I saw her, September 26th, attending to family cares, and apparently well, with scarcely a trace of the pleurisy effects remaining on auscultation. I have no doubt whatever that, without the operation, she would have died.

July 28, 1868, I saw a young mechanic who was said to have

\* Why not suppose a telectasis exists to a certain depth over the whole lung?

had "liver complaint" in the spring, and since then had been unwell. He had dyspnœa and cough, and pain in the left side. When I saw him he had orthopnœa, and had been confined to his bed. He was emaciated, and appeared very ill; it was thought he would not recover. I found evidence of pleuritic effusion into the left side of the thorax, with dislocation of the heart, some bronchial respiration and mucous râles in both lungs, some being distinct in the left back, just at the usual point of puncture. I felt it necessary, however, to explore, because it seemed as if he would certainly die soon unless relieved. I feared hopeless disease—but I felt equally sure that it was right to give the chance of relief by tapping. Accordingly I introduced the trocar in the usual place in the back, but where I heard distinctly tubular respiratory murmur and unequivocal râles. In fact, the severity of his symptoms, and my knowledge of the innocuousness of the operation, led me to introduce a trocar in a spot which, according to the usual interpretation of physical signs, I should have avoided. Nevertheless I drew out thirty-four ounces of serous fluid, with immediate and permanent relief. He began to improve forthwith. No attempt of reaccumulating occurred, and all the rational signs soon disappeared.

January 4, 1869. He had been at work two months. March 1, 1869. Scarcely any difference between the two lungs, and râles all gone.

*Second.—To prolong life, even with the severest complications.*

The following is a striking case of this nature. I was called to an old man, æt. about 70, who, after having had cardiac disease with hypertrophy for many years, was suffering with general dropsy of the legs and abdomen, and more recently an effusion had taken place in the pleural cavity. He had permanent orthopnœa, and certainly did not afford any rational ground for the hope of obtaining anything more than a temporary relief. I told him I believed that if we could obtain fluid, he would be materially relieved of his severe symptoms. At my visit he was a great sufferer, and the left pleura was full of fluid; a large quantity of serous fluid was immediately drawn off, with not only entire relief to the orthopnœa, but subsequently the effusions left the abdomen and legs, and the patient lived in comparative ease for two years afterwards, when he died of his cardiac disease.

*Third.—To shorten latent pleurisy.*

The three following cases are in point:—

A youth from New Orleans, long before the late rebellion, was taken ill with febrile symptoms, and was said to have "Southern fever." He gradually got better, but never recovered wholly, and, after months of deteriorated health in the South, he came up to the North to recruit his shattered constitution. When in New Jersey, he was taken down anew with febrile accesses, and was then said to have an attack of "Northern" fever. Finally, he arrived at the vicinity of Boston, and I was summoned in consultation, and was told the following facts. There had been no

thoracic symptom, and the sole fact that led his northern attendant to discover a pleuritic effusion was the statement from the aunt that during sleep, the youth seemed at times to breathe with difficulty. Accordingly the physician had examined, and was highly surprised to find proof that one pleural cavity was full of fluid. I tapped him the next day, my theory being that the fluid had been very gradually accumulating for months, viz.: from his first attack of fever. I drew off a large quantity of serum with entire relief to all the obscure symptoms that had been depressing him. In twenty-four hours the lung was fully expanded, and in less than six weeks the youth was careering around on horseback, in all the elasticity of perfect health, which he had not had from the time of his first attack.

An aged physician of Boston had had pleuritic effusion for several weeks. He had no severe symptoms; but knowing the exact condition, and being aged, he dreaded the idea of paracentesis. I finally persuaded him of its innocuousness, and of the probable relief he would obtain. I drew off an equally large quantity of serum with complete relief, the lung expanding afterwards freely. The recovery was rapid.

Finally, I will mention the case of an active merchant, aged 62 years. He had always been in robust health. He was inordinately fat. It was with the greatest difficulty, and only on the strongest pressure, that I could distinctly feel the narrow\* intercostal spaces.

Feb. 13, 1867. I saw him in consultation. The pleurisy had developed itself with great insidiousness, but auscultation had clearly revealed the gradual filling up of one pleural cavity. He had, at my visit, a short dry cough, which had shown itself chiefly during the preceding week. Previously to that time he had driven into Boston to business daily. I found him restless and nervous, and with a certain pressure of breath, but no pain or fever. Pulse 70. The left side was full, and the heart was thrust high up to the right, the impulse being in fact felt chiefly under the third rib. I proposed tapping immediately. The tube usually employed, though driven up to the hilt, was too short, as the motion of the patient constantly drew it out a line or more from within the pleural membrane. I was obliged therefore to get an instrument very much longer, and by forcing this as one would through a thick layer of fat pork, I finally succeeded, on the following day, in drawing from the chest several ( $3\frac{1}{2}$ ) pints of serum. Not a trace of evil consequence followed the two punctures. On the contrary, the lung slowly but surely expanded,

\* The books declare that in cases of large effusion the intercostal spaces of the diseased side are wider than those of the other. This perhaps is usually the case, especially when nature has "pointed," and an abscess is forming. But it is far from being so in very many instances in the period of the disease when I have tapped. On the contrary, there has often been, as in this case, a *contraction*, spasmodic apparently of the intercostal muscles, and the spaces between the ribs have been exceedingly small on the diseased side, while those on the healthy side have been much widened.



and in a few weeks he was beyond my attendance, and in six was at business in Boston, in spite of my requests for caution. He felt so well that he seemed unable to listen to cautions. He has continued well since.

I am sure, Mr. President, that you, and those of the Academy who know me, will believe that in detailing these facts I am not falsifying. Gentlemen, they are solemn and true statements, and as such should command your attention.

*Fourth.*—*I would use the operation to prolong life and relieve suffering temporarily, even if there be no hope of ultimate cure.*

The two following cases illustrate this :—A physician, æt. 70, had organic disease of the stomach and parts adjacent, and intercurrent with it, and probably consequent thereupon, was an effusion into the right pleura, causing at times frequently recurring dyspnœa. On this gentleman I operated eight times during less than one month. He experienced so much comfort from it that he called it “his luxury,” said that “for twenty-four hours after thoracentesis was performed he was lapped in Elysium.” On one occasion he requested me to perform it, but having operated only a very few days previously, I suggested waiting at least twenty-four hours, and when I visited him the following day he addressed me earnestly, in fact solemnly adjured me never again to refuse to operate when the patient, from previous experience, demanded it. I shall not soon forget that appeal from the really dying man.

The following illustrates the same. The natural language of relief was the only means we had of knowing of the comfort the operation gave.

A young woman, a teacher, became epileptic, and when I saw her her mind seemed almost wholly lost. She was lying in bed; partially propped up, but perpetually writhing and moaning as if in great suffering, but utterly unable to give intelligent answers. On auscultation we found signs of a large effusion into one pleura. I operated, not for the hope of cure, but simply to relieve her. One quart of pus was drawn. The poor patient lay down tranquilly and slept easily, and, strange as it may seem, even the mind seemed to recover somewhat from its prostration. She knew more what was going on around her, and expressed in many natural ways the real comfort we had given her. There was no attempt at reaccumulation, and at the autopsy, about a month afterwards, we found recent adhesions only, no fluid remaining.

*Fifth.*—*I think we shall hereafter operate on acute cases of common pleurisy which do not early yield to remedies after a few weeks of treatment.*

The question will often arise, I think, hereafter, whether, after treating a case for a few weeks and no attempt at absorption takes place, we should not try really the *abortive treatment* by actually removing the fluid, and allowing the lung to expand. Experience has not as yet decided at what period of the early time of the disease we may puncture. But I cannot see any valid reason for continuing any active treatment more than one, two,

or three weeks without puncturing. Certainly those cases in which I have had to operate early, owing to the dyspnœa, do not contraindicate this course by their results. On the contrary, they cordially invite us to try the operation as early as we please after the chest has any great amount of fluid in it, from any cause.

Gentlemen, I might cite other cases, but I forbear. I might tell you how in some few cases I failed to get fluid, owing, I have little doubt, to there being no fluid, but a simple membrane; and in one case a large tumor filling up the chest as fluid would. In other cases I failed to get it for some unknown reason. I am inclined to believe, however, it was owing to lymph at the end of the canula. At times, too, perhaps, the fluid has been too thick. Yet I think it will be rare that this condition will prevent success, for I have once drawn fluid when it was of honey-like consistence, so that it took time for it to come to a level in the basin where it fell. At times I have reversed the instrument, and on injecting a little warm water, success has been attained. But, at other times, by introducing the trocar at a point above where unsuccessful previously, the fluid has flowed freely.

Allow me, gentlemen, to allude to a few points of interest in reference to medical opinion on the operation at the present time, and to some of the objections urged against it, etc., before closing.

#### MEDICAL OPINION ON THE SUBJECT.

I think it may be said that medical opinion in Europe is still against the operation, and in this country it is not in favor of it. Gairdner,\* of Glasgow, fully endorses it, and justly says that this method of performing the operation will make a revolution in all our ideas of the matter. Aitken,† of London, also supports it, as did Budd, formerly of London. Fuller,‡ however, doubts about it, and urges objections, but would use it as a last resource. On the Continent I am not aware of any Frenchman defending it since Trousseau died; and Freirichs, of Berlin, Oppolzer and Skoda, of Vienna, have no faith in it. Within two months I have received a letter from a student at Berlin. He tells that he saw cases in Berlin allowed to die which would have been operated on, and possibly saved, in Boston. Flint and Peaslee, and F. Barker, of New York, and, I think, others, approve of the operation.

Most of the profession in Boston agree to it. But they nevertheless often wait too long. Some few still ridicule it. I have seen within three years one acute case prove fatal while the medical attendant and I were actually arguing about the propriety of *performing the operation*.

My own view may be expressed as follows: I consider it my duty, *at all times and all places*, to press hard upon the medical

\* Clinical Medicine.

† Practice of Medicine.

‡ Diseases of the Lungs. London.

conscience of this community the following propositions, which I consider perfectly well-established axioms for me to rest upon:—

1st. The perfect innocuousness of the operation, if performed carefully, and with due regard to the feelings of the patient.

2d. The certainty of relief following sooner or later, if fluid be removed.

3d. The certainty that it sometimes saves human life.

4th. We must put wholly out of our minds that it is to be used as a "*last resource*;" but, on the contrary, take the ground that thoracentesis (by Dr. Wyman's method, that is to say) should be used, whenever symptoms require it, as readily as you would use any other remedy, a cathartic, a blister, a subcutaneous injection, etc.

#### OBJECTIONS TO THE OPERATION.

I think it best to meet here and answer all the objections that I have heard urged against the operation.

It is said that it is of no use to perform the operation, because all uncomplicated pleuritic effusions will get well under other treatment, or under no treatment at all. But if we grant that this may be generally true, it is entirely denied as a universal proposition. Death, or long-continued disease, will at times happen in every one's practice who stubbornly holds to this opinion.

2d. It is asserted by some persons that complications should prevent us from performing it. Never was there a falser theory, —one wholly unfounded in fact. The cases reported above fully prove the error.

*No matter what kind or amount of complications exist, the more they are in number, the greater the reason for thoracentesis.*

3d. We may injure the lung with the instrument. Well! suppose we do tap the lung, what harm results? How often is the lung injured by broken ribs, and no evil comes? I have struck the lung at times—I have seen another draw blood from it—yet with no evil results.

4th. We injure the lung by forcing it to expand. I have never seen this happen, perhaps because I always cease to draw immediately that the patient begins to suffer *any really uncomfortable feeling*, such as stricture of the chest, etc.\*

5th. It is said we cannot draw all fluids. This is a poor objection. We can at least *try* to get fluid, but it is not a valid reason for refusing to perform the operation.

6th. We may admit air into the pleura. Suppose we do. No harm usually results. I have at times accidentally pumped in one or more ounces. No trouble ensued. It is true that if a *permanent* opening be made in a case where serum simply is in the

\* After my remarks, Dr. Geo. T. Elliot remarked that pneumothorax came on in one case in which he operated, and Dr. F. Barker said he had three times known of severe pain in the side after the operation. I ask myself if these gentlemen may not, in their earnest and honest endeavor to remove as much fluid as possible, have continued the suction too long.

chest, the serum will inevitably soon turn into pus. But there is no permanent opening made by the exploring trocar.

7th. We may excite pleurisy by pricking the pleura. This is an ignis fatuus of some theoriser, not in the least founded in fact.

8th. We *may* injure the intercostal nerve or artery. So we may, but I have never seen any evidence of having done so in at least 248\* operations.

9th. The instrument is imperfect. It won't always act; it does not always draw. All these objections are commonly not met with in case a man before each operation thoroughly arranges the instrument, and tries it, and knows that it is in perfect order. He will be foiled often if he do not use these precautions.

The question is often asked, *Where shall we puncture?* If I can, I always operate in the back between the eighth or ninth, or ninth and tenth ribs, below the scapula, on a line with its lower angle. I auscultate the healthy side, and always tap at least two inches above the lowest point at which the murmur can be heard. Generally, I think, between the eighth and ninth rib is the best spot.

#### HOW SHOULD WE OPERATE?

Make no incision of the skin, do not try to make any valvular opening, but, guided by the forefinger of the left hand pressed deeply into the intercostal space, plunge quickly and freely in. Be sure to go in *quickly* and *deep enough*, to get fairly into the fluid.

I thank you, gentlemen, for the courtesy which has allowed me to appear before you, and for the kind attention with which you have received these desultory remarks.

\* April 11—250 at present date.



IS CONSUMPTION EVER CONTAGIOUS,

OR

COMMUNICATED BY ONE PERSON TO ANOTHER  
IN ANY MANNER?

A PAPER PREPARED FOR THE BOSTON SOCIETY FOR MEDICAL  
OBSERVATION.

BY

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# IS CONSUMPTION EVER CONTAGIOUS, OR COMMUNICATED BY ONE PERSON TO ANOTHER IN ANY MANNER?

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THIS question may seem one of very little importance, because we all may be of one opinion, viz., in favor of the negative of the proposition. Yet it is one about which I have been interested, in consequence of some cases that have fallen under my notice during the last ten or fifteen years. These cases I shall relate to the Society; but before doing so, let me refer briefly to the views of the medical profession on this topic, historically considered. In all that relates to medical opinion previous to the present century, I shall depend on the learned work by Dr. Young.\* It would seem, from Dr. Young's statements, that all the chief authors who, previously to the last quarter of the last century, treated of the subject, were full believers in the contagiousness of consumption. It is also well known, that public opinion, which is always the reflection of a preceeding medical opinion, sustained that view, at least in the southern portions of Europe. The Italians, until a very recent date, have had certain quarantine and other regulations relative to those dying of this disease, as are common now in smallpox cases. Aristotle, according to Dr. Mason Good, states that this belief in the contagiousness of the disease was common among the Greeks. Galen regarded it as unwise to pass even a day with a consumptive. Morton†, 1619, Riverius,‡ 1656, and above all, a century later, the learned and sagacious Morgagni,§ 1761, would not dissect a phthisical patient's

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\* Practical and Historical Treatise on Consumptive Diseases, deduced from Original Observations and collected from Authors of all Ages. By Thomas Young, M.D., F.R.S., London, 1815.

† Morton. Phthisiologia, London, 1619, p. 185.

‡ Riverius. Opera Medica, Fol. Geneva, 1728, d. 1656, &c.

§ De Causis et Sedibus Morborum, Fol. Venice, 1761, Ex. xix., 53—xlix., 15.

body because of the risk he thought would be run of taking the disease. Dr. Withering,\* in 1775, claims that it is certainly infectious.

Subsequently to this long period of almost entire faith in the contagiousness of the disease, we have a period of doubt and of questioning upon the subject for about twenty-five or thirty years.

Dr. Cullen,† in 1777, dares not assert that it is never contagious, especially in warm climates, but he had never seen a case, in which it was decidedly communicated by one individual to another.

The Edinburgh professor evidently doubted the commonly received opinion, but was unwilling to take a bold stand against the idea of contagiousness, as those of the same school did in 1794 (i. e. seventeen years afterwards), who not only opposed the notion, but presented strong cases against it.‡

Dr. T. Reid,§ of London, 1782, allows that, in extreme cases, it may be slightly contagious, and young persons should not sleep in the same room with a phthysical patient.

During the same year, 1782, Bursieri|| says the Colleges of Physicians of Tuscany had declared their disbelief in its contagiousness; but this did not prevent government from taking precautions against it.

Emale,¶ 1783, thinks it so contagious that it is dangerous to draw milk from the breasts of a woman, as recommended by Chevillard and others, for the cure of consumption, for fear of the disease being given to the nurse.

Vogel,\*\* 1785, is quite convinced of its contagiousness.

Darwin,†† 1793, is a complete believer of its contagiousness, in *persons nearly allied*.

By these quotations—coupled with the fact that I find none, during the same period, decidedly opposed to the proposition—I think I have given some proof—first, of the very general consent to the proposition of the contagious nature of consumption, until the commencement of the last quarter of the last century; second, that from that time until about 1800, there was evidently great indecision. From 1800, with some very few exceptions, Medical Opinion and Public Opinion have been gradually becoming settled in opposition to the theory of contagion. No one will deny that Morgagni showed great weakness in being unwilling to dissect the body of a phthysical patient. In fact, no one would hesitate to regard the disease, as seen now, wholly innocuous to the degree feared by the great pathologist of the last century.

\* Account of the Foxglove. Birmingham, 1785.

† First Lines of the Practice of Physic, Ediub. 1777.

‡ Dr. Young. Supra, p. 360.

§ Essay on Nature and Cure of Phthisis Pulmonalis. Lond. 1782.

|| J. B. Bursieri. Institutiones Medicinæ Practicæ, 4 v. Venice, 1782. English by Brown.

¶ 4 v. Ediub. 1800.

¶ Emale. Journal Médicale, lxiii. 1783.

\*\* S. C. Vogel. Handbuch der Practischen Arzney-Wissenschaft. 1785.

†† T. Beddoes to E. Darwin, on a New Method of treating Consumption.

I may here remark that one wonders at the fears of Morgagni, when he remembers that Fabricius Hildanus describes several examinations of bodies of persons dying of consumption, and this more than a century before Morgagni lived.

The following resumé of the authorities on the subject since 1793 up to the present time, will include my third period, or that of nearly utter denial of the idea of contagion in connection with consumption.

Only two writers of note and of undoubtedly strong minds, and these Americans, can be found to defend the proposition of contagiousness. These two writers are nearly at the two extremities of the period now under consideration, viz.: Benjamin Rush,\* 1793, and Drake,† 1854—about a half a century later. Dr. Rush is fully convinced of the contagiousness of the disease, and quotes what I cannot but think rather apocryphal cases sustaining his views. Dr. Drake, without taking the affirmative decidedly, nevertheless evidently leans very strongly towards it; quotes facts, and argues analogically that such a proposition is quite in accordance with the fitness of things.

Meanwhile a certain number, especially in the earlier part of the century, are in doubt. Dr. Heberden,‡ for example, 1802, has not been able to make up a decided opinion upon the subject; but he had seen several cases in which he could not find any more plausible reason than contagion, from the patient having been constantly in attendance on another, and even sleeping in the same room. The very same year, Haygarth§ and Darwin|| decided against the proposition.

Wilson,¶ 1803, says it is rarely attributable to contagion, but it is wisest to avoid sleeping with a consumptive patient. Little is said by authors upon the subject till 1809; and from that period up to the present time, authorities may, with the exception of the two first (viz., Rush and Drake), be classed either as being in doubt, and, at times, very decided opposition to the idea of contagion; or they ignore the whole subject, and thus virtually seem to consider it beneath their notice.

In the first class we find Portal,\*\* 1809; Laennec,†† 1818; Cowan,‡‡ 1835; Davies,§§ 1835; AnceI,||| 1852; Watson¶¶, 1857; and

\* Medical Inquiries, 1793.

† Diseases of the Interior Valley of North America. 1854, Vol. 2.

‡ Commentarii de Morborum historia et curatione. Lond. 1802.

§ Haygarth. Young, p. 395.

|| T. Beddoes. Hygeia, or Essays Moral and Physical. Bristol, 1802.

¶ A. P. Wilson. Treatise on Febrile Diseases, 1803.

\*\* Portal. Observations sur la Nature et le Traitement de la Pleuro-Pneumonie. Paris, 1808.

†† Laennec. L'Auscultation Médiate. Paris, 1818.

‡‡ Cowan. Translation of Louis on Phthisis. London, 1835.

§§ T. Davies. Lectures on Diseases of the Lungs and Heart. Lond. 1835.

||| H. AnceI. Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula. Lond. 1852.

¶¶ Thomas Watson. Lectures on the Principles and Practice of Physic. London, 1857.

Cotton,\* 1858. All these writers actually oppose the notion. Dr. Watson rather dogmatically would settle the question by declaring that "a *diathesis* cannot be contagious."

In the second class are found the following well-known authors, most of whom have written special treatises on tuberculosis, and some of them have gone quite elaborately into the causes of consumption. Louis,† 1825; C. J. B. Williams,‡ 1834; Stokes,§ 1843; Blakiston,|| 1848; Copland;¶ Bennett,\*\* 1858; Lawson,†† 1861; Todd,‡‡ 1861.

From these quotations it is evident that, at the present time, we may say that Medical and Public Opinion are almost the exact reverse of what they were previously to the middle of the last century. My own doubts have disappeared under the close examination of my own cases, and of those reported by others. Nevertheless, I should not agree with the extremes of either party, but should hold, that while there is no positive proof of the real contagiousness of consumption, it would be very unwise to deny that some cases do arise, in which a close attendance upon and devotion to persons affected with phthisis is the prominent exciting cause of tuberculous disease of the lungs, in persons apparently healthy.

Similar views are held by others. In proof, I quote as follows: John Hogg,§§ 1860, remarks it is difficult to say if contagion can be fairly classed among the causes of consumption; but, he adds, it would be the *acmé* of imprudence to place a young person, not perhaps in the best of health herself, in close attendance on or near another. To this last expression of opinion I should give my adhesion, but I would not limit the remark to those who are not in good health—for I think our cases bear out the assertion that it is hazardous for even the most healthy to run that risk. A similar opinion is also held by Mons. Gueneau de Mussy,||| clinical successor to Chomel, in Paris. He declares, 1864, wholly against contagion, as usually understood, but at the same time believes that it is probably transmitted by coition and by very intimate relations with an individual during any of the periods of the disease. At the time of writing he had two cases in the wards of Hotel Dieu. He quotes Andral as coinciding with him. He has observed a trouble in the throat among

\* R. Payne Cotton. On Consumption, its Nature, &c. London, 1858.

† P. C. H. Louis. Sur la Phthisie. Paris, 1825.

‡ C. J. B. Williams. Physical Signs of Diseases of the Chest. Am. Ed., 1834.

§ W. Stokes. Diseases of the Chest. 2d Am. Ed., 1844.

|| G. Peyton Blakiston. Practical Observations on Diseases of the Chest. London. Am. Edition, 1848.

¶ Medical Dictionary. Article, Tubercular Consumption.

\*\* J. H. Bennett. Clinical Lectures. American Edition. New York, 1858.

†† L. M. Lawson. Practical Treatise on Pulmonary Consumption. Cincinnati, 1861.<sup>1</sup>

‡‡ R. B. Todd. Clinical Lectures. London, 1861.

§§ John Hogg. Practical Observations on the Prevention of Consumption. London, 1860.

||| Leçons sur les causes et le traitement de la tuberculose pulmonaire. Hotel Dieu, Union Médicale, vol. 4, p. 374, Nov., 1859.



those who live with the tuberculous. He thinks it more common for consumption to be transmitted from a man to a woman than from a woman to a man.

These general statements give a glance, at least, at the three periods of opinion, and the commencement of a fourth, on this subject; viz., 1st, the period of almost undoubting faith—extending from the earliest records of medicine up to 1775, or thereabout; 2d, a short period, of 25 or 30 years, of great and painful doubt; 3d, a time of utter scepticism; and, 4th, perhaps I may add, a modified faith, not coinciding with either the “everlasting Yea,” “the centre of indifference,” or “everlasting Nay,” as Carlyle would express it.

Let me proceed now to the examination of my own cases.

CASE I.—A gentleman died Feb. 13, 1858, after two years' illness, with confirmed and undoubted phthisis. I saw him and examined him, and know that he had all the marked rational and physical phenomena of the disease.

The wife consulted me March 4, 1864, with decided signs of phthisis. It appears that her family was usually quite healthy. Parents alive: mother 56 years old, asthmatic; father well. One uncle, by the father's side, had died of consumption. She was an only child, and had always been, up to the time of her husband's illness, in most robust health. In fact, she never “dreamed of being ill,” either generally or locally. She was her husband's sole nurse during his year's illness. She slept with him to the last. He complained if she left him. He wanted the warmth of her body to keep off his chills at night. Often her clothing was wet by the moisture from his body. Her parents protested against this course, and warned her of the consequences; but the wish expressed by the husband, her own confidence in her own health, and abandonment of all thought of self for her husband's sake; these considerations overcame all others, and she continued this course until the end of his life. When this time arrived, she found herself wholly worn down, and she had never been well afterwards. She had been much less capable of endurance, and was easily fatigued by comparatively light work. Every spring she had been worse than at other portions of the year. She had had severe cough from the first, with aggravations of it at times, but with only slight sputa. A year since, hæmoptysis occurred—about 3 i., bright, arterial. She had had more or less pain about the right shoulder; had been able to lie on either side, and had never been seriously prostrated. Her hæmoptysis occurred after the performance of a very difficult piece of music, requiring extra physical exertion and mental attention. She had had at times some dyspnœa; no palpitation, but was easily tired by walking. Her appetite had been fair, with good digestion as its attendant. Menses regular—urine variable, at times very light colored. She had had a series of periods of emaciation, and subsequently some gaining of flesh, but with a gradual loss of 25 pounds—having fallen from 150

to 125, her present weight. Chills and heat, but no sweating had occurred. This spring, for the first time, she had had hoarseness, and coughed, morning and evening, in paroxysms of about half an hour. She had taken iron and other tonics with benefit, and whiskey not regularly. When I saw her, her countenance was a little pale, not much emaciated, or showing the marks of severe disease. Pulse 88. Tongue clean. The physical signs were as follows:—There was flatness behind right clavicle, and dry crackling to the fourth rib. Voice a little resonant. Flatness also at the top of the right back, and a coarse crackle and slight resonance of voice. Rales, though less marked, even to the base of the lung, and respiratory murmur rather rough even there. Respiration in left lung noisy, somewhat sibilous.

She evidently had tubercles, slowly developing, at the top of the right lung. She considered that this was the culmination of a disease excited by her close attention to her husband during his illness. The facts of the mother's "asthma," and the uncle's death by phthisis, may throw some doubt as to the contact with her husband being the chief cause of the disease in her case. She was hereditarily (so the sceptics will say) tending to phthisical disease—and the over-excretion, &c., broke her down, and made her an easy victim.

I might, in reply, say, that if we take the simple fact of one uncle having died of phthisis as a proof of the *hereditary* disposition, and this in opposition to a most perfectly robust health up to the time of beginning to attend her husband, I cannot but think as much may be said of almost every family. There is scarcely one in this community in which some member, remote it may be, has not had symptoms of phthisis. The "*asthma*" of the mother does not really bear upon the subject of *her phthisis*.

CASE II.—Miss E., a young and very healthy-looking woman, I saw in November, 1855. She was the sole attendant of a very sick, tuberculous husband, and slept with him during all the periods of his disease. I was struck with her apparently robust health. A short time after her husband's death she had hæmoptysis. She is now, I believe, alive and married again, but is ill with phthisis, which commenced then, but which makes, however, very slow progress, and does not seem to very *materially* impair her health.

CASE III.—In 1855 I attended Mr. H., who was an invalid with pleurisy, and subsequently tubercular disease appeared. He remained for years, fighting against the complaint, and finally died with tubercular pneumothorax. His wife's mother died of phthisis, but she herself was splendid in her kind of robust English form, when her husband fell ill. She *devoted* herself without stint to him, and gradually began to "heck." On examination, physical signs were noticed under the clavicle. The symptoms rapidly increased, and she died before her husband, and was evidently tuberculous.



Those knowing the facts believe, as I believe, that if she had not attended upon him so closely and so long, she might be now alive.

CASE IV.—Miss S. sprung from a family, in which no trace of consumption is known ever to have existed. She was living as a farmer's daughter on a most healthy site, and enjoyed most strong and robust health, when she went to attend an invalid friend, to whom she was most tenderly attached. The friendship was mutual. The invalid sought for and received the closest attention on the part of her young companion. They slept in adjacent rooms at night, and the strong one acted as nurse at all hours, day and night. This attendance commenced in July, 1853, and continued till June, 1854, eleven months, when, at the request of her parents, our patient went home. In August, however, she again spent a week with her friend, and was with her at her death, August 30, 1854. During all this period the invalid did not wish any one else to do aught for her, and her young companion often lifted her when the patient was too feeble to raise herself. During one of these efforts Miss S. felt that she had "strained herself." Ever after she had an uncomfortable "coldness" at the epigastrium and a tendency to chilliness—with, subsequently, in the spring of 1855, some dyspepsia. In the following months, her parents noticed her rather feeble condition, but did not regard her as seriously ill. She was treated as a dyspeptic, and considered herself as such. She had scarlet fever in the spring. In August, 1855, cough set in, i. e. after twelve months' invalidism, and the cough continued uninterruptedly till death. Diarrhœa commenced in March, 1856. She died of phthisis, July 27th, 1856, nearly two years after her friend's decease. I saw her once in the spring of 1856. She then had the signs of anomalous tuberculosis, i. e. crepitus, a pure tubular respiration, and solidification of the lower two thirds of the right lung. She also had had two abscesses under the arms, a few days before my visit.

CASE V.—A gentleman whom I was attending in advanced phthisis, in 1855, assured me that, though one brother had died of phthisis, after some particular exposure, he did not deem himself hereditarily disposed to the disease, as no other case had occurred in the family. This gentleman attended constantly on his wife, who died of phthisis. On my asking whether he thought that attendance had had any influence on his own health, he remarked instantly, as if the idea had previously occurred to him, that the *first* marked paroxysm of coughing he had ever had, and from which he dated all his own ills, was when he was raising his wife in bed and inhaled very deeply her breath. It caused much irritation at the time, and coughing was produced, which continued afterwards.

CASE VI.—Mrs. ——— I saw, October, 1854, aged 30 years. She had been taken suddenly with hæmoptysis. She had had five attacks of the same previously—the first at 18 years of age. Usually they had been of slight duration, and in the interval her

health had been excellent. She had slight disease at the top of both lungs. She had been the sole attendant of her husband, who had died about two years previously of phthisis. She had often risen in the night and given him her warm and dry night dress, and moved him to her side of the bed, while she lay down upon the mattress, wet with his sweat, and with only a blanket thrown over it. This was only one of the numerous acts of self-sacrifice which she performed. She told me she knew she was running a risk, but she did not, at that time, care for it. From the time of her husband's death her own health had been materially changed—she had never been as well as before. After contending against the disease ten years, and being at times able to labor much, as superintendent of a large mantilla establishment, she is now in a hopeless state of disease. But she is fully convinced that the close attention to her husband first undermined her health.

CASE VII.—Mrs. ———. This is doubtful as to present diagnosis—but I fear phthisis will result. She has no hereditary tendency, and is a stout, full built woman, and was, till lately, healthy. She attended her husband, who died of phthisis in June last, 1863, after nine months' illness. She was his sole nurse. She slept with him till the last, and scarcely left his room for three or four months preceding his death. She was, of course, fatigued, but went into the country and felt sufficiently well, except she had a liability to headache, to which she was not subject before. In November or December, 1863, she first noticed some dyspnœa; afterwards some palpitation and some sweating at night. These symptoms increased, and she was thought to have phthisis. I found evidence of latent pleuritic effusion filling the whole of the left chest. Tapping was performed Feb. 9, 1864, with relief to the dyspnœa. Between three and four pints of serous fluid was drawn out, but there has been no frank improvement of the local signs. The lung has never freely expanded, but, on the contrary, dulness has always remained, and only the finest crepitus or cough is produced at upper part of lung.

May 14, 1864.—Since this was written, the lung has expanded more, and the fluid that remained has been absorbed. There is, however, still an obscurity of murmur and much less resonance than usual.

CASE VIII.—I saw two sisters in the central part of Massachusetts. I visited them in October, 1863. The parents cough. The grandmother, by the mother's side, had two aunts and a sister die of consumption. Evidently the disease strongly tends to occur in the family. One of the present sisters was dying of advanced tuberculous disease; she had had slight cough for two years, but only broke down two months before I saw her, and apparently from over-exposure, at a celebration on the return of soldiers to her native town, she being chief manager. The whole of left lung had the fine crepitus of recent tubercles, and there were signs of a few at the top of the right. The sister was devoted to her, and declared she would

not leave her. She watched with her, and would not take even the slightest precaution. I warned her of danger, but she paid no heed, careless of everything save attention to the sick one.

The physician has written to me, within the past six weeks, that she, too, is beginning to show signs of phthisis, and is, in reality, more rapidly sinking than her sister.

#### CONCLUSIONS, FROM THESE CASES.

Out of the 8, we have 7 females, viz., 5 wives, 1 sister, 1 female friend; and only 1 male.

They may be divided into three classes, according to their hereditary tendency to phthisis.

1st. Possibly, phthisical tendencies previous to exposure, though apparently healthy at commencement of attendance,	}	5
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2d. No such tendencies,	}	2
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3d. Doubtful, but appeared healthy,	}	1
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In 1st class, only one had had symptoms previously, viz., hæmoptysis, years previously, without serious influence on the health. In the four others, one or more members of the family had had phthisis; viz., one mother, one uncle, one brother, and one sister.

The opponents of the idea that contagion had anything to do with the production of phthisis in the 1st and also in the 3d class, viz., in six of the eight cases given, will argue that these cases are utterly worthless as a ground-work of any argument for contagion, and therefore they should be put wholly out of the question. I cannot entirely agree to this view of the subject, when I remember that, without exception, my patients were all, apparently, in good health before they fell under the supposed deleterious influence of close attendance upon the consumptive friend; and that from the termination of that attendance the health of all was found to be undermined. Hence I really think I can claim, with a certain degree of reason, that, whatever were the hereditary tendencies, the attentions in nursing the phthisical friends were the *exciting* cause of their own disease.

But let us examine the two remaining cases:—And here also we must admit a certain lameness in one of them as a basis of argument, viz., a doubt about the *certainty* of the diagnosis. So we are really reduced to only one perfectly reliable case, in which no trace of tuberculosis can be found previously to the exposure. Now in this case, if we can ever allow ourselves to speak of causes in diseases, the phthisis and subsequent death were not only a sequence, but I think and others think a veritable consequence, in the strictest relation of cause and effect, of her previous close attendance on a tuberculous patient. But was there necessarily contagion in this case? By no means, the anti-contagionists will reply. It may well be questioned whether anything at all similar to contagion, as we

usually understand that term, can be predicated of it. There is proof enough of weakened health, of dyspepsia, and its usual concomitants, and after months of gradually deteriorating health phthisis did set in. But then the same result might have occurred without any attendance on any body. It is the most common way for phthisis to develope itself. Confinement in any bad atmosphere, ill ventilated apartments, anxiety and depression of mind, irregular digestion, have often produced identically the same results. Then, again, they may say—If contagion had anything really to do with it, why did it prove so long in showing itself? Usually contagion shows itself soon. This was months in developing itself.

Dr. Drake meets this argument, by saying that we really know nothing of the length of time necessary for the incubation of phthisical influences. One disease is promptly contagious, and runs its course rapidly. This is no reason that the same rapid course should be pursued in phthisis. On the contrary, all *its* phases are slow. Why not its period of incubation? In fact, have we not a proof of this in the years that pass over before an hereditary tendency bursts forth? The arguments against contagion drawn from the long period elapsing between exposure and the evident disease is therefore really of small moment. But it falls wholly, when we remember how hydrophobia and other deleterious influences may remain dormant for an indefinite length of time—how miasmatic influences may remain years unknown, and then suddenly show themselves.

But again, say the anti-contagionists, the nature of consumption is very different, certainly, from the majority of contagious diseases; there is no eruption as in smallpox, measles, scabies, syphilis, &c. Above all, the actual inoculations by Kostum, Le Pelletier, &c., proved abortive.

To this the contagionists might reply—But there is whooping-cough, mumps, some sore throats, pleuro-pneumonia in animals, &c., from which there is not apparently any morbid matter recognizable by the senses. Yet these are capable of producing their kind. In regard to actual experiments of inoculation, it is asserted that they were not satisfactorily performed.

Still further, the non-contagionists say—If contagion acts, why have we not had more cases in these many years, and why such a vast preponderance of married females amongst these patients? Does contagion usually select women? Why not rather suppose that the entire *abandon* with which women throw themselves into the duty of nursing; their total recklessness, at times, when attending on a dear friend, of the commonest rules of hygiene in regard to air, exercise, food and sleep—that these, conjoined with and acting upon an excessively sensitive nervous nature, are the causes of the disease in those cases?

We may thus argue pro and con upon the subject, and yet not arrive at exact and definite opinions, as to the precise matter in dis-



pute; and I will close this part of my paper with the single remark, that the strongest argument I know against contagion, is the fact that, considering my facts at least, woman is more liable than man to be taken. Why this, if there be not something at any rate more important than contagion to explain the phenomena of the case? The really contagious disease spares neither age nor sex. There must be something, therefore, more than mere contagion.

#### FINAL STATEMENT OF VIEWS.

Let me say that I commenced this investigation strongly prepossessed with the idea (gained while attending my patients and with a cursory knowledge of their antecedents) of the essentially contagious nature of phthisis, under certain circumstances. The force of my own facts, when thoroughly examined, has led me materially to doubt the correctness of my former impressions.

I will lay down the following propositions, which I believe correct.

*First*.—Consumption is not *contagious* in the usual acceptation of that word.

*Second*.—It may be *infectious*, and to this extent only. By long attendance of the closest kind, by inhaling the breath of the phthisical patient, by living in the phthisical atmosphere, so to speak, and in general by a neglect of hygienic laws during such attendance, the health may be undermined and phthisis set in. How far the depressing passions may have an effect, it may be impossible to say; but that they exert an important and deleterious influence, we cannot deny. Doubtless the want of exercise and the neglect of all hygienic laws have an immense influence. But as we see all these influences, except the supposed infection, existing often, for a long time, without producing phthisis (for example, a long attendance on patients ill with other non-tubercular diseases), I do believe that, in some few instances at least, we must admit a degree of real infection in our estimate of the matter. Especially must we take this into consideration in the case of a sister or a brother in a family, where the hereditary tendency to phthisis is strong; for in this case I doubt whether it would be safe to allow the sister to attend as nurse, even with the best moral and physical hygienic influences.

I am confirmed in this view by two more considerations. We see carcinoma and other chronic diseases have never even suggested this idea of contagion to the medical profession, at least so strongly as phthisis has. Is not this an argument in favor of the idea of there being something peculiar in tubercle, whereby it is capable, at certain periods of its development, of producing its kind? Second, I deem of some importance the positive statement made to me by a husband, the sole male patient among our cases. He distinctly asserted that the first real cough arose when he was raising his wife in bed, and consequently was making an effort and drew in a deep breath, inhaling at the moment while his wife was exhaling. The

chief part of the air he drew in was loaded with emanations from her lungs. As she was, at that time, in the last stages of phthisis, those lungs were probably ulcerated, and possibly some irritating matters may have escaped with the expired air, and have been inhaled by the husband.

#### PRACTICAL CONSIDERATIONS, ARISING FROM THESE CASES.

While we may feel assured that contagion, as held by Morgagni and the Italians, is, at least in this country, a delusion—we may feel, I think, equally assured that we should warn a wife or a sister or near female friend from devoting herself too closely to the attendance upon a consumptive husband, sister or friend. For the sake of the attendant as well as the sick one, sleeping in the same bed, or even in the same room, should be avoided. If, however, this cannot be prevented, then we should try to eliminate all deleterious influences as much as possible. The room should be thoroughly ventilated and cleansed daily. The attendant should eat regularly and pay especial attention to the digestive system. The diet should be nourishing and simple. It would not be amiss for her to take some wine daily. She should walk daily out of doors, and not confine herself to the sick chamber week after week without release. In actual attendance she should beware of inhaling the breath of the invalid. Disinfecting agents might, with advantage, be used in the spittoons. In a word, every good hygienic influence should be brought to bear upon the attendant. At the same time I would have the community understand that, with such care, there is scarcely a trace of danger in any case, save that of a sister, or a wife whose husband is so miserably exacting or thoughtless as to demand that she should sleep with him during his illness. The danger of the health being injured in these latter cases is great, even if the above hygienic rules be strictly adhered to. The best plan is not to allow a sister or a wife ever to take the *sole* charge of the invalid. Whenever it is possible, let a regular hired attendant assume all the harder work, and sleep in the room, if need be, at night, or, still better, in an adjacent room. The wife or sister should be in the room only during the day, and even then with proper and regular intervals of removal to the fresh air. These rules it may often be impossible to carry out; still oftener, they *will* not be. Nevertheless, I believe our cases fully show that they are simply rational and right, and rules that the contagionist and anti-contagionist ought alike to adopt.

WHILE these sheets are being bound, I find in the London *Medical Times and Gazette*, Dec. 14, 1867, page 651. a reference to an article in the *Journal de la Société de Statistique de Paris*, by Dr. Gross, of Berne, on the "Geographical Distribution of Phthisis."

This is a very valuable document, and in it I find the following remarks. After having alluded to the fact of the comparative immunity from phthisis enjoyed by those living on high ranges of mountains, Dr. Gross says—

"But there is an atmospheric element which does exercise an unfavorable influence in respect to the prevalence of phthisis, and that is *humidity*. Nearly all the countries and localities in which phthisis has been noted as frequent, are distinguished more or less by great humidity, whilst those that are free from the disease have generally a very dry atmosphere, either by reason of their great elevation or by great degrees of cold."





# CONSUMPTION

IN

NEW ENGLAND AND ELSEWHERE,

OR

SOIL-MOISTURE ONE OF ITS CHIEF CAUSES.

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B O S T O N :

DAVID CLAPP & SON, PRINTERS.....334 WASHINGTON ST.

1868.



[These "Prefatory and Historical Remarks," prepared as an introduction to a small second edition of my address on "Consumption in New England," have been printed separately, 1st because they contain the essential points of my whole argument for the existence of a law of soil-moisture as an important cause of Phthisis in New England and possibly elsewhere; 2d, because they contain the latest facts upon the subject, viz., returns from England proving that *artificial drying of the soil by subsoil drainage, &c., has actually caused a decrease of the death-rate by phthisis* in places where such sanitary measures have been adopted.

H. I. B.]

## PREFATORY AND HISTORICAL REMARKS.

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IN 1862 I published the following address on Consumption in New England, as it was delivered at the annual meeting of the Massachusetts Medical Society, in the same year. In the preface I used the following language:—"The force of facts presented to me by correspondents compels me to resign the commonly received opinion that consumption is *equally* diffused throughout New England, and to adopt the new one of the address, viz.: that there are some spots which have very little of that scourge of the human race, while in other places and even in particular houses it prevails to a frightful degree; and moreover that these spots may be perhaps within a very short distance from each other. Again: facts, as I believe, indicate that *dryness of the soil in the surroundings of any place is the prominent characteristic* of the former, or of *places comparatively free from consumption*; while *dampness of the soil characterizes the latter*, or as they may be aptly called, *consumption breeding districts*."

Long before this publication, however, viz., in 1855, and again in 1856, I had made reports to the same Society to the same effect.

During a visit to Europe in 1859, I laid these facts before

several persons on that side of the Atlantic. Among these were Drs. Farr and Greenhow of London, and the members of the London Society for Medical Observation ; \* Dr. Gairdner of Glasgow, Dr. Bennett and Sir James Y. Simpson of Edinburgh, and Monsieur Louis at Paris. Some of these gentlemen expressed a deep interest in the results at which I had arrived. By conversation, I endeavored to induce the making of similar investigations in England.

Subsequent to the publication of the address I received several letters, and from some of these I take the liberty of quoting in order to show the opinions expressed therein.

That excellent man and wise physician, the late Dr. John Ware, wrote thus to me :—

*Pavilion, January 11, 1863.*

MY DEAR DR.—I have but just had time within a few days to go over your pamphlet in such a way as to satisfy myself. I think you have made out your case as satisfactorily as any point in so obscure and complicated a subject as the causes of disease, can be made, and it is surprising to me that any one can hesitate to admit that you have made out a very strong case—at least strong enough to be made the foundation of advice in practice. I do not hesitate to act upon it, and should consider myself criminal not to do so. \* \* \* \*

I am always very assuredly your friend,

*Dr. Henry I. Bowditch.*

JOHN WARE.

The following from the late Dr. Coolidge, of the United States Army, I deem of especial value, because in his very

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\* As this statement may not be confirmed by the records of the Society, I think it right to say that, strictly speaking, I did not present my observations to the Society while in session, because the communication was not considered a legitimate subject to be laid before that body. But I was allowed to present it to some of its members previous to a regular session of the Society.

able Army Medical Reports\* he had arrived at similar conclusions, deduced from Medical Reports that had been forwarded to the Surgeon General of the United States, from all the military posts of the Union.

SURGEON GENERAL'S OFFICE,  
*Washington City, D. C., March 6, 1863.*

DEAR DOCTOR,—\* \* \* In tracing the connection between moist or wet soils and consumption you have gone one step, and an important step, beyond my own published conclusions, but my professional experience, and, I may add, my sad personal experience, corroborate your views, and make me willing to endorse them fully. \* \* \*

Yours, with much respect,

RICHARD H. COOLIDGE.

*Henry I. Bowditch, M.D., Boston, Mass.*

Dr. Farr, the eminent medical gentleman so long connected with the Registration of England, I quote simply to prove that I laid the subject fully before him in the published address, as I had in 1859 verbally presented it to him.

*London, March 17, 1863.*

MY DEAR DR. BOWDITCH,—I beg to thank you for your valuable address on Consumption.

We have a series of tables in progress which will assist you in your researches. They show the mortality from consumption at different periods of life, in each district of England and Wales, during the years 1851–60. \* \* \* \* \*

I am respectfully yours,

W. FARR.

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\* Statistical Reports of the Sickness and Mortality in the Army of the United States, compiled from the Records of the Surgeon General's office from 1839 to 1854. By Richard H. Coolidge, M.D., Assistant Surgeon United States Army. Washington, D. C., 1856. Same from 1855 to '59. Washington, 1860.

Under date of June 18, 1863, I received véry important testimony from the author of one of the ablest works on Climatology that has yet appeared in Europe, viz., A. Mühry, M.D., of Göttingen.\*

*To Henry I. Bowditch, M.D., Boston.*

SIR,—You certainly have given the proof, that even in local distribution this almost “ubiquitous” disease prefers wet soil to dry soil, or, as I should like to express it, that the persons affected with tubercles of the lungs are exposed on moist and damp localities to the bad effect which such a soil inflicts in the greater part of the diseases, even if or though not producing them. This is a fact hitherto not sufficiently known and not taken into consideration in the treatment of the consumptives. It is in conformity with the geographical distribution of consumption. I take occasion to say that I now firmly believe in the wholesome effect of dry climates on the absence of phthisis; formerly I was unable to decide on this point, because in the East Indies the disease is rare, and the climate not dry (though in winter it may be called so). But I am now of opinion that though there may be exceptions, generally among the good effects of the dry climates belongs the absence of phthisis. In the Sahara and in the interior of Asia it is very rare or wanting. I have given this as my opinion, in a book which was published some years after the *Noso-Geography* (1856), in “*Allgem. Geograph. Meteorologie*,” 1860, p. 144, Note. \* \* \* \*

I have the honour to be, Sir, Yours sincerely,

A. MÜHRY.

*Göttingen, 18 June, 1863.*

From the tenor of the following remarks, in a review of my address, in the *London Medical Times and Gazette*, Feb. 27, 1864, it will be evident that English Medical Opinion

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\* *Klimatologische Untersuchungen*, &c. A. Mähry, M.D. Leipzig et Heidelberg, 185

had little confidence in the results to which I had arrived. After giving the title of the address, the writer says :

“ This pamphlet, though received a number of months ago, from being accidentally laid aside, has not yet received a notice in this journal, for which apparent indifference we owe Dr. Bowditch an ample apology.”

Then follows an analysis of the main features of the address. The writer closes in these words :—

“ Whether our author’s zeal in the study of his subject has permitted him to overlook other causes that may influence the one in question or not, does not affect the value of his book, which will amply repay one for the time spent in its perusal.”

My “ zeal,” to which the writer thus kindly alludes, was the result of a *firm conviction* that I had arrived at a great truth which I wished to impress upon my hearers and my readers—a truth, moreover, forced upon me by facts, and in opposition to my own previous views—a truth, as I thought, of vast importance to every village and township, nay, even to every family in New England, and *possibly elsewhere*.

Scarcely two years ago, Dr. Martin, author of an admirable work on the influence of Tropical Climates, \* wrote as follows :

*London, 37 Upper Brook Street W., }*  
December 20th, 1865. *}*

MY DEAR SIR,—\* \* \* \* I have much too long deferred thanking you for your great kindness in sending me a copy of your very interesting and important address on “ Consumption in New England.”

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\* \* Influence of Tropical Climates on European Constitutions. By James Ranald Martin, F.R.S. London : 1856.



The subject is in a great measure new to us on this side of the water, and is therefore regarded with much interest, with a hope that it may be further investigated. \* \* \* \*

I remain, my dear Sir, yours very faithfully,

J. R. MARTIN.

*To Dr. H. I. Bowditch.*

Meanwhile, I obtained from time to time, from private individuals, facts confirmatory of my views, and the Rhode Island Registration sustained them. Little progress, however, was made in the spread of them.

Finally, observing that the first question proposed for discussion by the International Medical Congress that was to be held at Paris in August, 1867, was on the causation of phthisis, I thought I might possibly induce that body to appoint a Commission, composed of able men in different quarters of the globe, to investigate the question which I deemed settled for New England. I accordingly sent the following letter to the Secretary of the Congress.

BOSTON, United States of America, May 27, 1867.

*To the Secretary of the International Medical Congress,  
called to meet at Paris during the month of August, 1867.*

SIR,—I see that one of the subjects proposed for discussion at the meeting is the causation of phthisis; or consumption as it is popularly called; its geographical distribution, &c.

I beg leave to present, through you, for the consideration of the Committee who may have to report on the geographical distribution of this disease, the accompanying address and documents.

The address was delivered by myself before the Massachusetts Medical Society at its Annual Meeting in 1862, and was printed in the Transactions of the Society (Vol. 6, Part 2, Second Series).

Massachusetts is one of the New England States. These States are six in number, and are the most Northern and

Eastern States of the Union. Massachusetts holds in the New England States a nearly central position—three of them lying north and two south of it. Situated as it is on the eastern coast of the Continent, its shores are bathed by the Atlantic and are fully exposed to all the winds that come from the east over that ocean.

It lies between  $41^{\circ} 15'$  and  $42^{\circ} 52'$  of North Latitude, and between  $69^{\circ} 54'$  and  $73^{\circ} 30'$  of Longitude west of Greenwich, England. Its coast, chiefly sandy or rocky, bends inward, forming numerous small harbors, and is between 200 and 300 miles long when following the various curves—but is less than a hundred if a straight line be thrown between its southern and northern capes. It extends into the continent in a nearly rectangular shape about two hundred miles from east to west, and is about ninety miles broad from north to south. Its surface is undulating with many lofty hills, but no very high mountains are found in it. The Connecticut River runs through nearly its centre from north to south, and it has a few more rivers of lesser size, which with numerous rivulets and ponds provide ample supplies of water.

Every diversity of soil may be found, from the dry perfectly arid sand, down to the moist meadow.

The climate is very cold in winter—quite hot at midsummer—chilly and damp in the spring—cool and more balmy in the autumn.

It is the type of New England in its natural peculiarities; and its inhabitants, in their social and industrial relations, are second to none of its sister States. Comfort and thrift prevail generally in New England. Pauperism is rare among the native born population.

Mr. Keith Johnstou, in his chart on the Geographical distribution of Disease, marks New England as the place where consumption is peculiarly endemic. That writer makes no mention of this malady prevailing otherwise than *equally* in all parts of New England. Such was deemed to be a fact until the investigations, the results of which I wish to lay before the Congress, were made by me, aided by intelligent physicians resident in all of the three hundred and twenty-five *townships* of Massachusetts.

I myself certainly held firmly to that opinion, and it was only after returns had been made from numerous towns that I was led to doubt the truth of it.

I *now* believe,

1st. That phthisis is very *unequally distributed* in New England:

2d. That there are some places which enjoy a very great exemption, if not quite as much exemption from its ravages as any portion of the globe can claim:

3d. That there are some spots, nay even particular houses, which are frightfully subject to it:

4th. That there is a cause governing this unequal distribution of the disease; a law not recognized before my investigations, and still practically ignored by the majority of human beings, which, however, is one of the main causes, if not the sole cause of this *unequal* distribution in New England, and possibly elsewhere:

5th. That this cause is intimately connected with and apparently dependent on moisture of the soil on or near which stand the towns, villages or houses in which consumption prevails.

That this law of soil moisture is a cause of consumption certainly in New England, and possibly in other parts of the globe, is proved in the paper herewith presented, by the following data:

A. By the Massachusetts State Registration Reports:

NOTE. (It may be added that the Reports made, subsequently to my investigations, by Registrars of Massachusetts, fully confirm my investigations relative to the unequal distribution of the disease, although one proposes to modify somewhat the law as laid down by me.\* The Registration of Rhode Island, a sister State immediately adjacent on the south, fully sustains my views. I beg leave to lay those reports also before your body):

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\* I do not, however, admit the cogency of his argument. On the contrary, I deny entirely even its relevancy to the point at issue. The assertion, even if true, that consumption decreases from North to South in this country, is no proof for or against the question whether dampness of soil causes phthisis. See Twentieth Report of Births, Deaths and Marriages in Massachusetts in 1861, pp. 54-7, by Oliver Warner, Boston, 1863.

*B.* By the Medical Opinion of Massachusetts as embodied in returns made to me (as a Committee of the Massachusetts Medical Society), from physicians resident in all the towns of the State, one hundred and eighty-three of whom give specific details bearing upon the subject:

*C.* By actual statistics of deaths received from such correspondents—the statistics being generally given before either my correspondents or I were aware of the prevalence of any law upon the subject, or in fact that there was any really *unequal* distribution of consumption within the limits of the State:

*D.* By peculiarities of certain towns where consumption is either prevalent or of rare occurrence, such differences being most clearly connected with corresponding differences of soil-moisture:

*E.* From statistics, sometimes reaching over half a century, in regard to families successively inhabiting certain well-known houses in Maine and Massachusetts—rather popularly celebrated as containing many consumptive patients; whole families having been, at times, cut down by the disease, while the reason for such mortality has never been suspected until after these investigations were completed:

*F.* By confirmatory facts and statistics from the sister States of Maine, New Hampshire, and Rhode Island, indicating that the same law exists over New England generally.

*G.* From medical statistics from the United States Army Reports, strongly supporting the possibility of the prevalence of the same law over the whole territory of the United States.\*

*H.* From the results of my own medical practice since being convinced of the truth of the law; which results prove that persons, having only the early signs of phthisis, viz., hæmoptysis, cough, &c., and general prostration, and without marked physical signs, have decidedly improved and finally recovered after moving from wet localities to well-selected dry ones; that persons having well-marked crackling under the clavicle, &c., with the rational signs of phthisis, have at times rapidly improved, and have at length recovered wholly

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\* Dr. Coolidge's Report, cited above.

or in part, under the same circumstances of change; and finally, that cases of well-advanced phthisis have had prolonged life given by following the same course.

From the writings of various authors on the proper selection of a place of residence for consumptives, I have tried to find out whether or not the same law prevails over the entire globe.

This part of the subject I have, of course, been obliged to treat very imperfectly for want of data. But on consulting the various authors alluded to, I find as follows. Pau, Villa Franca, Nice, Mentone, Malta, Egypt, Madeira, Canary Isles, the Azores, Bermudas and Bahamas, Undercliff (at the Isle of Wight), St. Croix, St. Thomas, West India Islands, and parts of Mexico and Peru, have each and all essentially dry soils, and, though some of them are liable at times to heavy rains, the soil, being porous, becomes very quickly dried up. All these places are well known places of resort for consumptives. On the contrary, Pisa, Naples, Milan, Mantua, Verona, Exmouth and Sidmouth, &c., are all stated by authors to be prejudicial to consumptives, and certainly public opinion never sends patients to them. These all have essentially moist soils.

All these points I present with great deference to the Assembly of my honorable and accomplished associates at Paris. Although from the correspondence I have had with one or more eminent climatologists in Europe and America, I am aware that they are disposed to consent to my general deductions, I know that the subject has never been investigated in the broad manner that is proposed in the programme of the Commission that calls the International Congress into being.

While thus presenting my views, I trust with becoming modesty, I should do injustice to my own firmest convictions if I did not earnestly press them with their accompanying proofs upon your candid consideration, inasmuch as I believe that in them are contained evidences of one of the primal laws of the development of phthisis, not only in New England but perhaps over the whole surface of the earth. This latter and equivocal part of the question I ask the Congress to take measures for settling either affirmatively or negatively, on the firm basis of actual observation and research.



I regret extremely that circumstances beyond my control prevent me from joining personally in your deliberations.

I therefore send this letter, and documents accompanying it, by the hand of the Commission appointed by the Suffolk District Medical Society (an integral part of the Massachusetts Medical Society), to represent it in your body.

I remain respectfully yours,

HENRY I. BOWDITCH,

President of the Suffolk District Medical Society,  
Professor of Clinical Medicine in Harvard University,  
Member of the Society of Medical Observation at Paris.

The result of my efforts was simply an abortion. The Secretary and the Congress virtually ignored the whole subject—as the following letter will perhaps sufficiently indicate.

BOSTON, DEC. 12th, 1867.

*To the Editors of the Archives Générales de Médecine.*

GENTLEMEN,—In the October number of your excellent Journal, page 496, you use the following words: “En outre, la commission avait reçu les travaux du Dr. Bowditch (de Boston), de Mons. Carnuff (d’Ontario), sur la Tuberculisation en Massachusetts et en Canada. Ces diverses travaux établissent que les mauvaises conditions hygiéniques, la mauvaise alimentation, le manque d’air, de lumière, d’exercice, sont les principales causes de la phthisie.”

Your reporter could not have looked at my communication, or at the papers which I forwarded to the Congress—otherwise I am sure he would not have used the language given above. The sole object of my communication was to prove that in Massachusetts, and probably in all New England, and possibly in other parts of the world, *a residence on, or near a wet soil*, is one chief cause of phthisis. I desired that the question should be investigated with reference to a much larger extent of country, than I, or any one person could reach.

I believe that this law of *soil-moisture*, as a cause of phthisis, exists elsewhere than in New England, but it has



not been thoroughly investigated by any one. I hoped that this investigation would have been undertaken by the International Congress. But your reporter has put me in the absurd position of one writing a formal communication to a learned body upon what seem to me self-evident propositions. In my communication I made not a single allusion to any of the causes of phthisis mentioned in the above extract. I know of no one who would deny the truth of each and all of them.

I feel especial regret that the Congress took no notice of the suggestions contained in my communication, because I see that independent observations have recently been made in England by Dr. John Simon, medical officer of the Privy Council, and given in his last official report.\*

These observations, based on the official mortuary statistics of twenty-five towns in Great Britain, as investigated by Dr. Buchanan, prove that in *these towns, which had been thoroughly drained so as to be comparatively dry*, instead of wet places as originally, the most striking fact resulting is a *marked diminution in the number of cases of death from phthisis*.

This result in Old England, thus far, fully sustains what I had proved for New England, where I have found, First, that from twice to three times as many more phthisical cases occur in the wet than in the dry localities; and, Second, that about in proportion to the amount of damp soil near or in a locality will be the number of phthisical persons found in it. Statements similar to this I openly defended in an address delivered before the Massachusetts Medical Society in 1862, having made partial reports to the same effect several years previously to the same body. Under the circumstances, may I ask you to correct the erroneous statements of your reporter?

Respectfully, yours,

HENRY I. BOWDITCH, M.D.

P. S. By this mail I take the liberty of sending my address mentioned in this letter.

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\* The Ninth Privy Council's Report on Public Health. London, 1867.

The English investigations mentioned in the preceding letter are referred to in an editorial review, in the London Medical Times and Gazette (Sept. 28, 1867), of the Ninth Report of the Medical Officer of the Privy Council, Dr. John Simon. The Report itself has not yet reached this country.

It appears that Dr. Buchanan had been directed, in 1865 and '66, "to investigate the effect of drainage works, and other sanitary regulations designed to promote the public health." It was deemed important to prove to the popular mind the value of those works and regulations. "Therefore twenty-five towns, containing an aggregate population of 600,000 souls, were selected as the places where structural sanitary works had been most thoroughly done and had been longest in operation." These were visited by Dr. Buchanan, and he has given with much patient detail the nature of the experiments with their results, as shown in the mortuary statistics of the different towns. Among the results are the following. He found a reduction of the death rate in the majority of the towns. He then examined with reference to various classes of disease, and observed various results, more or less curious and important, but which, as being not germane to our subject, I will not mention here, but pass immediately to that bearing upon the etiology of consumption. It was discovered that there had been but little apparent influence produced on the prevalence of all pulmonary affections excepting pulmonary phthisis. And upon this latter I prefer to quote the exact words used by Dr. Simon, when reporting to the Privy Council Dr. Buchanan's results.

"These facts appear to indicate a partial dependence of pulmonary phthisis on some of the unwholesome conditions which have been removed, and when detailed examination is

made of the cases that give that indication, and they are compared with the different class where phthisis has not lessened in its amount, the *novel and most important* conclusion suggests itself that the *drying of the soil* (my italics) which has in most cases accompanied the laying of main sewers in the improved towns, has led to the diminution, more or less considerable, of phthisis. The facts which are yet in evidence seem most strongly to support this conclusion, which, should it be substantiated, will constitute a *very valuable discovery*, evolved by Dr. Buchanan from the inquiries here reported upon. It will be seen that the reduction of phthisis where certain works have been executed is far too large and far too general to be regarded as an accidental coincidence, and the fact that in some of these cases the diminished fatality of phthisis is by far the largest amendment, if not the only one, which has taken place in local health, becomes *extremely interesting and significant* (my italics) when the circumstance is remembered that works of sewerage by which the drying of the soil is effected, must always, of necessity, precede, and sometimes indeed precede by years, the accomplishment of other objects (house drainage, abolition of cess-pools, and so forth), on which the cessation of various other diseases is dependent."

Dr. S. considers the results so important that, by his advice, the Privy Council has directed further inquiry to be made on the subject. Thus, at last, it appears that the "very valuable discovery" and "novel and most important conclusion" has been arrived at in England, that by an artificial drying of the soil of towns the mortality-rates from phthisis is lessened!

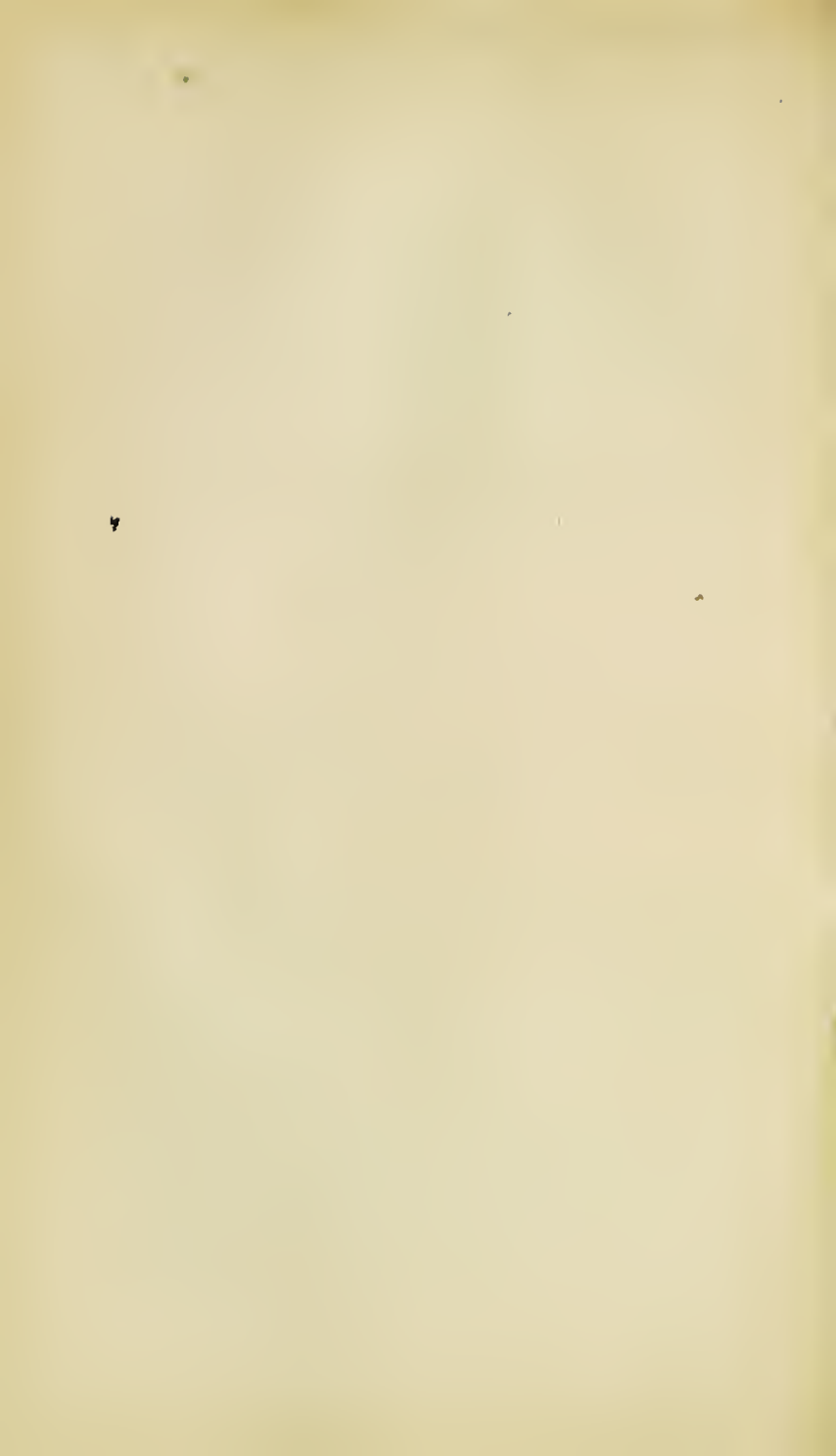
The Privy Council of England has now taken up the subject, and we may confidently expect from that august body reports that will be deemed worth consideration by all.

For myself, I rejoice that the authorities of at least one great nation have now fairly commenced the investigation of this question.

The benign influences of such an investigation cannot be overrated. I believe it will be of immense importance to those now living, but of far greater good to future generations, when what is now to most minds comparatively speaking a problem to be solved, shall have been adopted as the abiding thought and rule of action of the citizens at least of Old and of New England. At present, I think that every medical practitioner in New England, who, like Dr. Ware, conscientiously investigates the matter, ought to feel it "criminal" not to act upon the idea that a residence on a damp soil is liable to bring with it inevitable injury, a shortened life, and perhaps death itself, to the consumptive under his care.

HENRY I. BOWDITCH.

113 *Boylston Street*, Jan., 1868.



# HIPPOPHAGY.

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DURING my recent visit to Europe, I was requested by a professional associate in a neighboring town, to learn all that I could upon the use of horse-flesh as a food for man, and subsequently he embodied in a letter, from which I take the following extracts, the reasons for his request.

“In my morning visit to the meat-shop, I have for five years observed the poor (generally women) looking for something in the name of meat, cheap enough for them to buy. After hunting a while, they go away empty, or get a piece of bone with very little meat on it, or a salted junk, too often salted only because previously nearly spoiled by age. Many of these customers are known to me, and as I thought of their children who really needed meat, I was often led to think whether anything could be done to provide cheaper food of the same nature.



“The horse was, of course, thought of; and then there came up the other side of the question, equally human, or at any rate, humane—namely, the welfare of the horse in his old age.

“Our main street is one of the avenues to Brighton. Once every week our eyes, our ears are distressed with the passage to and fro, driven by cruel men, of frame after frame of what were once the proud nags of rich men; and let any one go to Brighton of a market day, and he will be impressed with what must have been their worth and position in earlier days.

“What is meaner than to see a horse that has been worn out in our service, starved and abominably abused till he dies? *Old* horses—the best of them—usually end life after this fashion.

“In fact, a class of cruel, brutish men get their miserable living from work extorted from them. This item of the suffering of *old* horses I have never seen put as it should be. If these old and tortured animals can be fattened and eaten, let the humane men attend to it.

“There is a practical objection, I am told, to eating horses in this country, because they are so costly, that it would not pay to fatten, until age had destroyed their value.” . . .

The above extracts contain in embryo all the questions involved in the subject. When first proposed to me, the matter seemed trivial. As I have investigated it, it has become more important.

I propose, therefore, to discuss it chiefly under the three following heads :

First.—The history of the use of horse-flesh as food by various people in different countries. Under this division I shall present evidence

*a.* That in refusing to eat horses, the western, northern, and southern Europeans, and civilized North and South Americans, are an exception to the general rule of mankind over the globe.

*b.* That even in Europe, it was the common food of the people, and considered especially appropriate for sacred feasts and for pagan altars, long before the introduction of Christianity;\* and finally, that it has been under the pressure of necessity, used by Europeans on various occasions, not only without injury, but with absolute good to those who have partaken of it.

*c.* That it was priestly domination that drove hippophagism out of use among the Germans, about the sixth century of the Christian era; and it was a love of Christianity, mingled with much worldly wisdom, that stopped its use in Iceland, four centuries later.

Second.—I shall give a brief history of the introduction again of horse-meat as food into modern Europe, under the directions of the constituted authorities of nearly all the continental powers, and especially I shall refer to the very recent use of it in Paris, and of the rapid progress of hippophagism in France, notwithstanding the various objections urged against it.

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\* Laing (Translation of *Heims Kringlia* or Chronicle of the Kings of Norway, by Snorre Sturleson, vol. i, p. 85,) says: the best established of religious practices of the Odin worshippers, was the partaking of horse-flesh at the sacred festivals, "as commemorative of their ancestors;" and again. "hippophagism was the test of Pagan belief, as baptism is that of Christianity." Hence Saint Olaf in the eleventh century punished hippophagism with death.

Third.—I shall have some remarks to make upon the question with reference to the necessity in Europe or America for the use of horse-meat as food.

## FIRST PART.

### HISTORY OF THE USE OF HORSE-FLESH AS FOOD BY THE VARIOUS NATIONS THROUGHOUT THE WORLD.

Hippocrates, in his work on diet (*Περὶ Διαιτήσεως*) says, that it was used in his time, and that it was a “light” article of diet compared with other food, [p. 76.]\*

Xenophon recites that during the Retreat of the Ten Thousand Greeks, the soldiers found in the deserts of Mesopotamia a wild ass, whose flesh was like that of deer, but of a more delicate flavor, [86.] (Xenophon-*is Scripta*, Weiske’s, Leipzig, 1799, tom. 3, p. 25.)

Galen objected to it, but rather as a matter of taste than of salubrity, [76.] (*De quadrupedibus: Usus in cibis.*)

Pliny says, (*Nat. Hist.*, lib. 8, ch. 69,) that Mæcenas taught the Roman epicures to use asses’ meat. It was a source of pride to Africa that she produced that species of “game,” [86.]

In Persia, according to Oelschlager (*Olearius en*

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\* Almost the whole of this part of the subject I have gleaned from a very learned work by Isidore Geoffroy St. Hilaire, entitled “*Lettres sur les Substances Alimentaires et particulièrement sur la Viande de Cheval.*” Paris: Victor Masson. 1856. 12 mo., pp. 261. The numbers in brackets refer to the pages of his work.

Moscovie, Tartarie, et Perse, Schleswig, 1647, French translation, 1656, p. 511, t. i,) and to Kerr Porter, (*Travels in Georgia, Persia, Armenia*, t. i, p. 460, 1821,) the same taste prevails, and the excellence of this food is proverbial. Quarters of the wild ass are sent as presents to friends, as haunches of venison are with us, [p. 89.]

In Africa, Marmol, (*Africa*. French translation by M. Perrot d'Ablancourt, 1667, t. i, p. 50,) states that a similar usage prevailed, and wild horses were also taken. Mungo Park, (*First Voyage in the Interior of Africa*, Castèra's translation, 1800, t. i, p. 166,) confirms this statement. [93.]

Phillips, [93,] (*Voyage en Guinée*, pp. 215 and 228, etc. ; and *Histoire Générale des Voyages*, 4to, t. iv, p. 353,) asserts that in Juida, in Africa, the negroes raise up a small and very intractable species of horse solely for food.

The Moors near Tunis and Algiers, eat their own horses, mules, and asses, [94.] Monsieur Lucas, member of a scientific commission on zoology, spent many months in these places, and often partook of this food, and preferred it to beef procured there.

Herodotus states, that in Asia horses and asses were eaten from the earliest times by all classes. Horses and oxen, chamois and asses were roasted whole on birth and feast days. The custom prevails as widely now as formerly, from the extreme East to the Ural mountains. [97, 98.]

The Chinese physicians object to the use of horse-meat, and yet they give most absurd directions in re-

gard to eating it. Hence the Chinese eat all horses they can get, whether they are killed, or die of various diseases, or from old age, [98.] (Duhalde. Description de la Chine et de la Tartarie Chinoise, t. 2, p. 138.)

According to Pallas and others, the Tartars, generally the inhabitants of Russia in Asia, and even those to the north and east of Europe, formerly had this same food, [100-105.] (Pallas' Voyages, t. i, p. 376; Beauplan's Description d'Ukraine, 4to, Rouen, 1660, p. 83; Huzard, art. Cheval, Encyclopédie Methodique Dict. de Médecine, t. iv, 1792, p. 694.)

According to H. Cloquet, (Faune des Médecins, t. iv, p. 74, 1823,) the Asiatics and Mongolians of the present day use it.

The Calmucks regard horse-meat as superior to every other, [101.] Bergmann, Nomadische, Streifereien unter den Kalmüken, 12mo., Riga, 1804, 2d part, p. 76.)

In Wetteravia, (part of Germany,) according to Montgomery, the nephew of Buffon, sausages and good soup are made of the same. This writer also states that the Cossacks likewise drink the blood, after having made the horse race hard, [105.]

The line of Virgil,

"Et lac concretum cum sanguine petat equino,"

refers to this fact. Georg., lib. 3, and Horace (Ode, lib. 3, vol. iv,)

"Et lætum equino sanguine Concanum,"

points to similar customs among the ancients.

Martial (de Spectaculis, 3) and Sidonius Appollina-

rius, (op. 1614. Paris. Sismondi ed., p. 318,) confirm the same in reference to the Sarmatians (the ancestors of the modern Cossacks of the Don,) and the Getans, (the predecessors of the Transylvanians, Moldavians, etc.,) along the eastern Danube of those times. In other words, there is ample proof that all these people, most of them Asiatics, and others borderers on Europe, are now or formerly were, hippophagists, [105.]

Coming now to the Celts, that Indo-Germanic people, that gradually occupied parts of France, Spain, Scotland, and Ireland, we have still more conclusive proof that they ate horse-flesh. This proof is derived from two famous edicts fulminated from Rome against hippophagy. Both are addressed to St. Boniface, the apostle of Christianity in Germany, and were intended to counteract the influence with the new converts of their former sacrifices to Odin, the memory of which was kept alive by this food, as already mentioned. Hence Gregory III. [107] (vide correspondence de St. Boniface, 1605, by Serravius,) sent his missive. The Pope wished by this interdict to show his abhorrence of all pagan worship and of things connected with the sacrifices. His holiness declared that such an hippophagic banquet was "*immundum et execrabile*," and penance was justly due for such an act. No anathemas could, however, overcome the love that the Germans had for this ancient food—"imprimis in deliciis," as Keysler calls it, (De Interdictu Carnis Equinæ Usu. Antiquates, etc., Hanover, 1720, page 321, etc.) So earnest were even the best converts against the giving it up, that



Zachary, St. Gregory's successor, sent another papal bull forbidding not only the eating of the horse, but also of the beaver and the hare. The two latter were, however, soon allowed; and only the flesh of the horse was still forbidden, because especially pagan in its associations. These various influences gradually drove the use of this food out of Europe—"magno detrimento rei familiaris," says Keysler, [109.]

Its use, however, continued for at least two or three centuries later in Iceland, and it would appear that there the love of it was so great that the priests made an exception in favor of it to some of the new converts. Perhaps there never was a more worldly-wise provision to produce conversions to Christianity than that which was made on this occasion. In the celebrated *Kristni Saga* of Iceland, (Ampere, *Literature et Voyages Allemagne et Scandinavie*, Paris, 1833, page 404,) is the following statement: Thorgeir, the lawgiver, and chief of the republic of Iceland, called all the people together, and said to them, "All the inhabitants of Iceland ought to be baptized, and to worship the same God." As to the custom of exposing infants (*exposer les enfants*,) and of eating horse-flesh, these will be allowed; so also a man will be allowed to sacrifice in secret, but if any one sacrifices openly and before witnesses, he shall be banished for a certain number of years. Ampere remarks, "that this singular agreement was made in the year 1000. All the inhabitants were baptized in the warm springs of the Geyser, and some years after that there was no open and avowed pagan in Iceland," [242.]

Passing over to Oceanica, we find that Marsden, [97] (*History of Sumatra*, Parraud's edition, 1788, t. 2, p. 188,) asserts that not only it is used as food, but is considered a real delicacy there.

Similar reports come from travellers in America. Wild and domestic horses are used [94] by several of the people of South America.

For example, Azara, (*Histoire Générale du Paraguay*. French translation by Moreau Saint-Méry. 1801, t. ii, p. 302,) states that the wild horses of the Pampas furnish food to the uncivilized Indians in their wandering life, [94.]

Sir Francis B. Head, Bart., (Rough notes taken in some rapid journeys across the Pampas and among the Andes. John Murray, 4th edition, p. 63,) confirms the above, and draws the inference, "I sincerely believe that they, the Pampas Indians, are the finest set of men that ever existed under the circumstances in which they are placed. They are all horsemen, or rather pass their lives on horseback." \*

In Brazil, many tribes use this food, [95] and Mons. Alcide d'Orbigny gives similar accounts relative to the Patagonians.

In Bolivia, the natives prefer horse-flesh to all other

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\* Sir Francis, alluding to their immense strength as superior to that of the civilized man, adds: "They are also very brave, and war is their occupation. They are entirely naked, yet they bear the burning heat of summer and the freezing cold of winter. . . . They have neither bread, fruit, nor vegetables, but subsist entirely on the flesh of their mares, whom they never ride. In their wars they stop for the night, and for food they kill a mare." Comparing this with civilized warfare, Sir Francis thus graphically writes of the advantage these Indians have over more civilized people. "On a long march it seldom happens that the bullocks are able to keep up with our men, whereas the food of the Pampas is flying always before him."

food, so declares Mons. Delvaille. (*Usage Alimentaire de la Viande de Cheval*, 8vo, Paris, 1856.) [95.]

In Chili the same record is given [96.] (Frezier, *Voyage de la Mer du Sud*, 4to, Paris, 1716, p. 67; and Buffon, *Histoire Naturelle*, Supp., t. iii, page 46.) They prefer the flesh of the horse to other food.

By Europeans, this food has been always used under certain exceptional circumstances, even in these latter days.

According to Huzard (vid. Parent de Chatelet's Report to Prefect of Police, entitled *Recherches et Considerations sur l'enlevement et l'emploi des chevaux morts*, 4to, Paris, 1827,) during the French Revolution a part of the meat used by the Parisians for six months, was horse-flesh, and some used it constantly. No ill effects resulted.

The famous Larrey, Napoleon's Surgeon-in-Chief, used this food in several of the hardest of his campaigns. On the Rhine, both he and his soldiers found it good. In Egypt he used camel and horse-meat, and during the siege of Alexandria, to the very greatest advantage. It became, in fact, the most powerful means of curing an epidemic scurvy. In the Austrian campaign he not only used horse-meat, but salted it with gunpowder for want of common salt. (Note 1434.)

In *Souvenirs Militaires de 1804 à 1814* (Par M. Le Duc de Fezensac, Général de Division, *Journal de la Campagne de Russie*,\*) we find several statements confirmatory of the above. In the account of the terrible

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\* London Quarterly Review. Littell's Living Age, Nov. 23, 1867.

retreat from Moscow, he writes: "In a cold and dark night these exhausted men threw themselves down at the edge of the fir forests, and there lit their fires and roasted horse-flesh in the blaze."

M. de Bausset, the Imperial Prefect of the Palace, harlequin, as it were, of the staff, with a certain grandeur in his mean epicurean appetites, during that horrible rout, complains that even the horses of his own carriage were stolen by the soldiers for food.

Sir Robert Wilson, who was on the Russian staff during the same retreat, says: "Thousands of horses lay groaning, half dead, and with large portions of flesh cut from them to feed the famishing."

Again it is stated that at the last attack by the Russians at Wilna, "the French had still some horses remaining, for all of them had not been devoured."

In the Crimean war, the late Dr. Baudens, General Health Inspector of the army, having read St. Hilaire's work, persuaded two batteries of artillery, encamped at Baidar, to eat horse-flesh, and they were less decimated by disease than other portions of the army.

At the same time Monsieur Decroix, at present the able chief veterinary surgeon of the Paris Guard, was in the French army and stationed near a corps of English soldiers, and he assures me that the whole air of the English camp was tainted by the putrefying masses of half buried dead horses, and while the wounded soldiers were breathing this impure atmosphere, they were also nearly famished for want of food, which these carcasses would have afforded them.

In the Morocco war, at a subsequent period, his

own horse fell, apparently paralyzed, after a very long day's journey. M. Decroix felt that, instead of leaving the poor creature to starve, when the corps would move on in the morning, it would be better to kill him forthwith. Having done so, he cut off a steak. The soldiers looked on, astonished. Nevertheless, finding he appeared to relish it, others soon followed his example, and in a very short time every part of the animal was disposed of in a like manner, and apparently much to the satisfaction of the soldiers.

We have thus made nearly the whole circuit of the globe, showing the same fact existing among various and most diverse people. We have shown that the horse has been not only the aider but the food of mankind in the chief parts of the entire globe, and that in some places it is raised solely for food.

Even in Europe, it was used for a long time, and in many localities. It is so used now at the north and east, in Germany. Heretofore, we have thought that the use of horse meat was exceptional and abnormal, and found only among a few nations. Should not the terms of this proposition be reversed? The exception is with us. St. Hilaire says, "The anomaly belongs only to the most civilized nations—nations that, with all their industry and science, have been unable to produce meat enough for their own people, while at the same time they sacrifice to an absurd prejudice what they have abundantly within their own reach."

## SECOND PART.

## RESUMPTION OF HORSE-FLESH AS FOOD BY THE NATIONS OF MODERN EUROPE, AND UNDER GOVERNMENTAL REGULATIONS.

For the last half century it has been more or less used in Denmark.

In 1842, (*Note sur le progrès de l'Hippophagie, etc., par M. E. Decroix, 1865, page 4,*) we learn that Dr. Perner, of Munich, began to resist the prejudice against this food, and, owing to his efforts, it has been authorized and regulated by the Bavarian Government. Other German cities have followed this example.

1847, Mons. Isidore Geoffroy St. Hilaire began the discussion of the question from his Professor's chair, at the Garden of Plants. His constant declaration was, "There are millions of Frenchmen who eat no meat, and yet, every month, thousands of kilogrammes of healthy, agreeable, and very nourishing food is used for secondary purposes or actually thrown away for manure." [p. 5.] At first, he was simply ridiculed. Soon objections were urged, but they were easily met.

In 1847, it was eaten by Pastor Bodeker, at Hanover, who continued to do so for several years, as an example to his people.

In 1854, it was publicly sold at Vienna, Usage (*alimentaire de la Viande de Cheval. Par M. le Dr. Blatin, Vice Pres. Soc. Protec. des Animaux, Paris.*)

In 1857, so many were in favor of using it in Paris, that a petition was sent to the authorities for liberty to open shops for the sale of it. Though the Board of Health advised the measure, it was not allowed.



In 1860, the Medical Society at Algiers made a similar request.

In 1864, the Paris Society\* for the Prevention of Cruelty to Animals made the same request of the Government, which was referred to the Minister of Agriculture, who, after consulting the Board of Health, authorized the sale.

Prizes were then offered to the first butcher who would open a shop for this object.

Meanwhile large hippophagic banquets were held in various German cities, in France and Algiers.

At Vienna, Berlin, in Wurtemberg, Bavaria, Baden, Saxony, Hanover, Schaffausen, at Lausanne, at Vilvorde, in Belgium, have arisen butchers' shops for the sale. At Vienna, during the first three years, no less than 4,725 horses afforded millions of pounds of meat.†

Finally, this last year (1867), that is, after twenty years of discussion, &c., the first shop was opened in Paris. The sale rapidly increased. The Society for the Protection of Animals, and Sisters of Charity, now daily distribute large quantities to the poor, gratuitously, collections being made to defray the expense. During the past nine months no less than eighteen shops have been opened, one recently in the very heart of Paris.

Among the most zealous of the propagandists of this food is Mons. Decroix, already alluded to, and now chief veterinary surgeon of the military of Paris. He has been called the "Parmentier of Paris," as he seems destined by his example and active zeal to force the Parisians to give up their prejudices in regard to this food, as Par-

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\* Decroix—cited above.

† Blatin—cited above.

mentier a century or more ago persuaded the French to use the common potato. He believes that example is the surest method of inducing people to do anything, and, therefore, he eats horse-meat frequently at his own table. Beef, mutton, and fowl, are rather exceptional articles, the horse-meat being his most frequent meat. In his intercourse with Parisians and strangers, he often invites them to dine with him upon it. It was my good fortune to be his guest on one of these occasions. The party consisted of two Parisian gentlemen, an American friend, and myself. It was a dinner party that I shall not soon forget; and, on the part of our French friends, quite full of that *piquante* vivacity found only in France. We sat from six until half-past nine p. m., and had all the varieties of bouillon, bouillie, roast, stewed, a *la mode*; dried meat, sausages; potatoes fried in lard from the horse; and fine crispy cakes made with that instead of butter. The oil procured from the horse was pure and clear, and almost odorless as the best of olive oil. I could not have recognized any difference between the two. We closed with salad dressed with that instead of olive oil!

#### OBJECTIONS AGAINST ITS USE.

During all the discussions that have arisen in Europe upon this aliment, I find the following objections, which, as they will be raised everywhere that the question is mooted, I will allude to at this time. Some of them fall from their own inherent absurdity, and not one of them is really tenable, or of importance, as the following summary will show.

1st. *It is unhealthy.*—The fact of its use in most of the large cities of Europe, and that no disease has resulted, is a sufficient answer for us at the present day; but the objection was used formerly with effect in Europe. The arguments given in the previous part of the paper; the facts of the free use of horse-meat by persons when under great difficulties in revolutionary and war times; the experiments at the Veterinary School at Alfort; and the personal use of the food by various individuals, in diverse localities, in later times, are proofs positive enough for reasonable persons. The fact that we have always eaten animals of the same class; and that of all animals, none has nicer cereal or vegetable food; and that none is so careful of his food as the horse is; and that he will not take anything that is not perfectly clean; these circumstances would serve to indicate that men who can eat the flesh of the filthy hog ought not to object to that of the horse. The whole life of the hog is occupied in sucking in the vilest of juices of the excrements of men and animals, and of the refuse of decaying vegetable and animal matter. He seems born simply to make manures of offal. We have all smacked our lips over a sparerib, and yet doubt about using the flesh of horse, which, *à priori*, under the physiological laws of digestion, would seem to be a more proper aliment than that of other animals who eat a greater variety of food.

Actual examination proves the healthiness of these animals which are used at Paris, that is, where a proper inspection is made. Monsieur Hazard, (*Hipophagie, ses Rapports avec l'Hygiène Publique*, 1867,) quotes

from a letter from Mons. Pierre, Inspecteur des Abattoirs á Chevaux at Paris, in which he says, that "of 2,765 horses inspected and sold for food, not one had an appreciable amount of disease that would have proved injurious to man." The increased demand for this food proves its innocuousness. Sisters of Charity urge its use to the poor who cannot get other meat to eat. One of them told me that it seemed to her that it was borne *more easily* by the stomach than common beef, and was more nourishing also. She had seen a child who seemed in a complete state of emaciation and debility, and unable to bear common food, revive, but, finally, it recovered on the soup and meat of this animal.

Chausier (page 189, Geoffroy St. Hilaire,) in 1803, made a report in behalf of the medical faculty, and on a request from the Prefect of the Seine that the faculty would decide whether dead horses could be safely given to pigs, and he declared it was perfectly healthful food. Parent du Chatelet, (Hygiène Publique,) in 1835, answered to the same effect for the Committee on Public Health.

*Second Objection.*—The taste is so peculiar, say the objectors, that a prejudice will always exist against its use. In answer, one may say that if we always argued in this manner, how can we account for the general use of very many articles of food, or drink, or simple luxury, which at first are rather distasteful, and which are now nearly universal. Tobacco, and many kinds of liquors, certainly require some effort before they can be used.

But the fact is, that those who complain of the taste,

are very often those who have tasted it only *in theory*. It is very difficult even for the initiated to distinguish it from beef. The first steak I ate was the juiciest and most tender article I ever tasted. Knowing what I was eating, I thought there was a slight "gamy" flavor about it, but of that I was in doubt. The most ludicrous stories are told in Paris of the mistakes made by various individuals, and from these narratives, as well as from my own experience, I am led to believe that few, if any, persons would be able to recognize the distinction, by taste, between beef and horse-meat.\*

*Third Objection.*—It will cost too much to fatten the horse, and we cannot raise him for food alone.

I should deny this positive assertion. Why might not the small race of horses used in certain parts of Africa—too small and indocile for labor—be acclimated, and used for food alone with us, as in their native country?

In reference to fattening the animal before killing, it may be affirmed that there is no need for so doing. The flesh is better when not fat. Generally, there is

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\* Among these narratives, the following is one of the most striking. A gentleman desirous of introducing an unwilling friend to the use of horse-meat, invited him to breakfast, with the understanding that a horse steak would be served up. Instead, however, of giving this, the host had a nice beef steak prepared, of which his friend very daintily partook, all the while protesting that it was tolerably good, but of that *peculiarity of taste* that would forever prevent its general use! No explanation was given, but three weeks afterward the same gentleman was invited again, and he consented, on condition that he was to have no more horse steaks. His friend replied that he would make all proper arrangements to gratify his visitor, and ordered for his breakfast a good steak from a horse. The conversation, of course, soon fell upon the previous meal, and the guest descanted on the excellence of the steak actually on the table, and of its vast superiority in taste over the former! After such a declaration, of course, a confession was made by the host, much to the astonishment of the epicure.

more oleaginous matter about all its ligatures than we find in the ox. The old animal is, moreover, nearly as good as young. The worthy pastor at Hamburg ate one thirty years old, and found it excellent.

Analogy here comes to our aid. Beef, or the flesh of older animals, is really better than veal, the flesh of the younger animal.

Still further. Were the horses used as food, doubtless many younger animals would come to the shambles, in consequence of lameness or accidents of various kinds.

*Fourth Objection.*—Prominent among the objectors to any innovation upon long established habits appear, with a few noble exceptions, the *savans*. This is quite in accordance with human nature as seen everywhere. But in Paris, these objectors in the Academy at first presented no argument or opposing fact, but simply reported that as horse-meat would probably never be used as aliment by any community, the discussion of the question was hardly proper for a learned body. Moreover the number of horses that would necessarily be brought to slaughter, would be so small, that this was another argument against a learned body taking action in the premises. Such arguments are too flimsy to deserve a moment's notice, and yet they were gravely used by learned men.

*Fifth objection.*—The use of horse meat would cause a jealousy among the butchers. This falls by its own absurd weight; for although in Vienna this was momentarily an obstruction, it was one of those impedi-



ments that only eventually urge onward the movement they undertake to impede.

*Sixth objection.*—Monsieur Astre, (page 170, Geofroy St. Hilaire,) brings forward as an argument a false statement, but thinks it overwhelming in its power, namely, that man had, for millions of years, sought for everything possible as food, but had never eaten horse, except from necessity. Now in what precedes we have the history of the entire world, including even Europe until Christianity became the predominant religion, to prove the false premises of the savant. His inference of course follows it. As St. Hilaire justly says: A European savant, talking thus, would be like an Arab who, because his people forswear pork as an aliment, should declare nobody in the world ever ate it, whereas if he were to cross the Straits of Gibraltar and visit Europe, he would find thousands eating this filthy animal, and wholly neglecting his own favorite horse meat. The European savant and the wild Arab would be alike in their folly.

*Seventh objection.*—This is still more extraordinary, and for a political economist and lawyer to make it, as does Monsieur Molinier, of Toulouse, [172] is remarkable. The gist of his plea is this, that if horse-flesh ever came to be eaten much, the price of it would be enhanced, so that the poor would be really but little better off than before.

The fact of its use, and increase of price, would be only a reason for its having been earlier brought into use, and far from being any argument whatever against its use at all. The argument amounts to this: if we

persuade poor people to eat horse-meat, there will be soon a demand for it, the price will be increased, and the poor will be unable to get it; *ergo*, says this learned judge, it will be better to let it all go, as now, to the dogs and the hogs! The principle to which he refers is, however, acting now in Belgium. The zoological gardens at Brussels paid fifteen francs for a horse in 1853, and fifty in 1855.

*Eighth Objection.*—Under this title I include a variety of weapons used in opposition to hippophagy by the public press of Paris.

*a.* Ridicule, which is so potent in France, has been used unsparingly. Of course, it is no argument.

*b.* It is said, and gravely, too, by one writer, that if we begin to eat our horses, by-and-by we shall have no horses for our carriages!

*c.* When all the horses are killed men will kill each other, and cannibalism will be the logical sequence of hippophagy!

*d.* The meat is less nourishing than beef. This is wholly denied, and the reverse of the proposition is held, by some writers, and by others who have used the food.

*e.* The flesh is tough. “Tough as horse-flesh” is proverbial from earliest times. But it may be questioned whether the proverb is true, and if true, whether it should prevent the poor from eating of the article provided they wished to do so.

*f.* It is said that only a small quantity of food can be thus gained. It would provide one fourteenth of all the food of France. But suppose it provided much

less, what argument is that against its use? Shall we refuse a few ounces because we cannot get pounds?

*g.* Admitting the feasibility of using horse-flesh, still we must not oppose such a time-honored prejudice. I have nothing to say upon this. It refutes itself.

*b.* The horse is subject to glanders, therefore we must not eat his flesh because we may be liable to take that disease. This is overthrown by the fact, proved by Rayer, that after cooking no disease is communicated to those who eat the flesh. Besides, the same argument would hold good in regard to all other animals, as all at times are diseased. [Geoffroy St. Hilaire, 185.]

*i.* The horse is the companion and friend of man, and, therefore, we will never eat him.

The answer to this is the question, what usually becomes of these faithful friends in their old age, for whom we have such reverential regard that we will not eat their flesh? Are they not usually sold when they can no longer work, to some vile miscreant who will drive them at times till they drop dead in the harness? Their last hours are tortured by over-work and by whippings to keep the poor creatures in motion while a single spark of life remains. This is no fancy sketch; and it is in order to prevent this inhumanity to horses, that all the societies for the prevention of cruelty to animals in Europe, except that of England, have urged hippophagy. This fact prevents ill usage in France. A single bruise or abrasion of the skin will prevent the sale of the animal. Self-interest, therefore, now prevents inhumanity. Even devils

become benevolent or saintlike if by goodness they can gain more profit to themselves.

### THIRD PART.

#### IS THERE ANY NECESSITY FOR USING HORSE-FLESH FOR FOOD IN EUROPE OR AMERICA?

Supposing there were no objections to the use of horse-flesh, what proof have we that it is needed either in Europe or America? Granted everything already claimed, namely, that if we should begin anew to use this food, it would be simply a revival of an ancient, perhaps excellent, custom; and second, that there is no objection to its use, either as a matter of taste, or of health, or in any other light; what, it may be asked, is the use of fighting a prejudice that, so far as all at present alive are concerned, may be said to be born with us, and has been strengthened with each year of life? Every one shrinks from the first notion of a horse steak, or horse soup; and even a French *cuisinier* cannot make that idea palatable. What use or necessity of talking more about it?

To the necessity of having more and cheaper animal food in Europe and America, let us now address ourselves.

For the necessities of Europe, I must again refer to St. Hilaire.\*

Vauban, [64,] chief engineer and warrior of Louis XIV., declared that the laboring population of France had in his day, that is, toward the end of the seven-

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\* Cited above.

teenth century, just about one third of the amount of food given to the cavalrymen.

Voltaire, in 1769, writes that the French peasantry rarely ate meat. A few of them have soup when ill. "They fast all the year."

La Grange, the great mathematician, in 1796, while making some investigations as to the needs of the Republic, declares that about one third less food was used by the soldiers of that day than is now deemed necessary.

Bouchardat, in 1848, remarking on Vauban's statement, says, that although France is better off at this present time than it was two centuries ago, it is still far enough removed from what is really necessary for health ; because even now, only a small proportion of laborers in France eat meat even twice a week.

St. Hilaire says, [page 31,] two hundred and fifty grammes of animal food is the daily need of a man in France, whereas he gets only about one third of that amount, [37,] and in the country, among the peasantry, only one sixth of the same amount, or even less than that : some eat it only a few times each year !

Well and truly may St. Hilaire write, (1856,) in presence of these appalling facts, at the termination of his very important work, as follows: " May this book, received by the public and by the administration, give the last blow to the absurd prejudice which I have been combating for nine years, and against which I shall contend, so long as I shall see under my own eyes the following spectacle : thousands of Frenchmen deprived of meat, or eating it only six times, twice, or

only once a year; and in the presence of this misery, thousands of kilogrammes of good meat abandoned everywhere to uses of secondary importance—given to hogs and dogs, or even thrown into the manure heaps.”

Surely here is enough proof that in France, and the same may be said generally of central Europe, more flesh food is needed. To meet this want, even partially, (one twelfth only of what is really necessary,\*) horse-flesh is proposed, and the societies for the acclimation of foreign animals are now turning attention to the still further demand.

What I have thus far given indicates that, 1st, there is a real want of fresh animal food in Europe; 2nd, that there is now a determined and apparently successful effort making in Europe to supply that want, at least in part, by introducing horse-flesh for food. But a further question immediately arises, Of what use are such discussions to us in America? The people here all have enough to eat. This everybody knows without inquiry. We see meat every day on the tables of the poorest. Such are some of the off-hand statements of men whose position seems to give them a right to speak so authoritatively. I cannot deny their statements, for, personally, I know nothing on the subject. I present, however, the following data procured for me by the kindness of four† physicians connected with the Dispensary. These gentlemen were requested to ask every patient, who applied to them during a week,

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\* Page 50, St. Hilaire calculates that 266,000 horses and mules die annually in France.

† Drs. Langmaid, Appell, J. Homans, and Knight.



how often they or their families ate fresh meat. From the data thus furnished, I was enabled to make the following table :

NUMBER OF DAYS IN THE WEEK THAT MEAT IS EATEN.			
No. of Persons or Families.	Very often; every day; four to five times a week.	Less than half of the time.	Not one day in a week; never; none for six weeks; every other week; every month.
133	133		
67		67	
12			12
<hr/>			
Total . 212			
Per cent.	62.73	31.59	5.68

I have only to remark that if out of 212 persons, (or families, as was, probably, really the fact,) so large a proportion as 37.26 per cent. of the whole, have meat less than half of the time, there may not be so much of fresh meat for the poor, even in this country, that we can properly reject the proffer of a greater supply and of a cheaper kind, provided it can be got without much difficulty. Moreover, that proportion, if it really represents the condition of the whole poor of this city, upsets the broad assertions given above, that the poor have enough to eat.

Some returns from the Massachusetts General Hospital, though smaller in number, were made with the greatest accuracy, and certainly apply to families.\* They seem to indicate that a very large proportion, over one half, of the poor applicants at the hospital, have less fresh butcher's meat than is really needed for aver-

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\* Dr. H. K. Oliver.

age human health, or what is usually deemed necessary for the sustenance of average human health:

NUMBER OF DAYS IN A WEEK THAT MEAT IS EATEN.

No. of Persons.	Six or seven times a week ; and at times twice daily.	Three times a week.	Less than three times a week, or very seldom.
11	11		
7		7	
19			19
<hr/>	<hr/>	<hr/>	<hr/>
Total, 37			
Per cent.	29.73	18.92	51.35

Certainly, if more than 70 per cent. of the families that applied for medical assistance at the hospital during one week, had fresh meat either very seldom or less than half of the time, is it not time to ask ourselves whether the condition of the poor of our city is all that could be wished for in this Christian land ?

By nationalities, these families were divided as follows:

Irish . . . . .	24
American . . . . .	7
English . . . . .	3
Scotch . . . . .	2
Nova Scotian . . . . .	1
	<hr/>
	37

In business or trades of the heads of families, they do not seem to have been from among the very poorest classes, as the following data indicate:

Laborers . . . . .	6
Mechanics (active) . . . . .	14
Mechanics (sedentary) . . . . .	4
Sailors and Fishermen . . . . .	3
Hackmen or Teamsters . . . . .	3
Women . . . . .	6
Doubtful . . . . .	1
	<hr/>
	37

Of the laborers, and widows, and washerwomen, twelve in number, none had a full quantity, that is, a daily supply of meat. Only two had meat three times a week, and all the rest had less than that.

Finally, bringing the two series of facts from the Dispensary and Hospital into apposition, we obtain the following tabular results.

Sources of Information.	No. of Families or persons.	Meat in plenty; very often; four, five, to seven times a week.	Meat less than half the time or scarcely at all.
Boston Dispensary . . .	212	133	79
Mass. Gen. Hosp'l . . .	37	11	26
Total, . . .	249	144	105
Per centage . . .		57.83	42.17

These combinations make it still more evident that the poor of the city of Boston do not have all the fresh meat that is desirable. That there is a very large number of persons, who daily beg for the refuse from the markets of this city, is well known to those who have meat stalls in these markets. People would not thus beg if meat were so plenteously supplied at their tables as some visitors of the poor believe.

In confirmation likewise of the same, I am permitted to quote the opinion of one of the oldest and most intelligent and devoted of the Sisters of Charity at Boston, who assures me that her opinion agrees fully with that of my correspondent's letter, namely, that fresh meat is a rare visitor to the table of many of our poor, and that even the salt meat they get is of the most inferior kind.

More extended researches I have endeavored to make, but a single individual can do but little. I have vainly

tried to get similar statistics from New York, but thus far without success.

What ought to be done in the premises? I have tried to lay a foundation; but what we shall raise upon it in this country is still an open question. Certainly it would be better for some of the poor to eat horse-meat than to eat no fresh meat at all. Will they do so? Meanwhile, will it not be well for all thinking persons to ask themselves whether it be not simply prejudice and ignorance, that prevents the community, as a body, from using this very palatable food?

#### WORKS ON HIPPOPHAGY CONSULTED IN THE PREPARATION OF THIS PAPER.

Lettres sur les Substances Alimentaires et Particulièrement sur la Viande de Cheval. Par Mons. Isidore Geoffroy Saint Hilaire, Membre de l'Institut, (Academie des Sciences,) etc. Paris. Librairie de Victor Masson. 1856. Place de l'Ecole de Médecine.

Un Banquet de Cheval. Epître aux Hippophobes. Par un Hippophage. Alger: Chez Tissier Libraire. Rue Bab-El-Gued. 1861.

Viande de Cheval. Communications faites à la Société Protectrice des Animaux. Paris: De Soye et Bouchet, Imprimeurs, 2 Place Pantheon. 1864.

1st. A pamphlet containing articles by Dr. Blatin, Vice-President, entitled "Usage Alimentaire de la Viande de Cheval."

2d. Les Préjugés contre l'Usage Alimentaire de la Viande de Cheval. Par M. Decroix, Vétérinaire en premier de la Garde de Paris, Secrétaire-adjoint.

3d. Viande de Cheval. Lettre adressée à Monsieur le Viconte de Valmer, President de la Société. Par Monsieur Bourguin, Secrétaire General.

Viande de Cheval. Note sur les Progrés de l'Hippophagie en France. Lue à la Société Imperiale d'Acclimatation dans la Séance, de 13 Jan., 1865. Par M. E. Decroix, Membre de la Société. Paris: Imprimerie de E. Martinet, Rue Mignon 2. 1865.

Usage Alimentaire de la Viande de Cheval. Banquet des Hippophages. Paris: Imprimerie E. de Soye, 2 Place de Pantheon. 1865.

L'Hippophagie, ses Rapports avec l'hygiène Publique et l'économie Société suivie des principales recettes pour la Préparation de la Viande de Cheval. Par C. Hazard, Membre de la Société Protectrice des Animaux. Paris: Imprimerie Victor Goupy, Rue Garancière, 5. 1867.

Lettre au redacteur du Belier. La Viande de Cheval et La Pomme de Terre. Rôle de la Lorraine dans ces deux initiatives.















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